

# Archaeological Hammers and Theories

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1983



ACADEMIC PRESS

*A Subsidiary of Harcourt Brace Jovanovich, Publishers*

New York London

Paris San Diego San Francisco São Paulo Sydney Tokyo Toronto

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ACADEMIC PRESS, INC.  
111 Fifth Avenue, New York, New York 10003

*United Kingdom Edition published by*  
ACADEMIC PRESS, INC. (LONDON) LTD.  
24/28 Oval Road, London NW1 7DX

#### Library of Congress Cataloging in Publication Data

Main entry under title:

Archaeological hammers and theories.

(Studies in archaeology)

Includes bibliographical references and index.

I. Archaeology--Methodology--Congresses. 2. Archae-  
ology--Philosophy--Congresses. I. Moore, James A.  
(James Anthony), . II. Keene, Arthur S.

III. Series.

CC75.A64 1982 930.1 82-11669

ISBN 0-12-505980-9

PRINTED IN THE UNITED STATES OF AMERICA

83 84 85 86 9 8 7 6 5 4 3 2 1

For Kate and Maura

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## Preface

The subject of this volume is archaeological method—the interaction of data and technique with theory and problems. A reader who consults it as a guide to *the* route from data to theory will be disappointed. Although chapters in this collection do address specific methods, there is a much larger issue involved. Archaeological methods have no validity independent of the context of their application. Methods, serving as our bridges between past behavior and an all too contemporary archaeological record, must be adjusted to fit the specific topography created by the interaction of problems, theory, and data. Of necessity, methods are tailored to their contexts, and are clearly subservient to problems. Methodology—the study of method—is not an autonomous branch of archaeological science; it will not enable us to create either context-free or theory-free methods. Beneath the seemingly reasonable goal of creating *an* archaeological method lies the unintended effect of defining the nature of archaeological data, the range of archaeological theories, and the scope of archaeological problems.

This volume represents a stock-taking effort; its subject is the relationship of archaeological means to social science ends. The methodological revolution, which we know as the New Archaeology, emphasized that the past was knowable—the chasm between the archaeological record and the behavior that created that record could be bridged through sheer methodological effort. For 20 years archaeology studied and contemplated itself. Archaeological epistemology, archaeological method, and the nature of archaeological theory—these are important paradigmatic issues, issues that define the discipline. Their resolution does not, however, illuminate prehistory or the motors of social process: it merely brings to light the cogs and gears of archaeological thought. It is through this effort that we have begun to transform archaeology into a science. This is a worthy project. It is important, however, to remember that science is a means and not an end.

Our aims here are several. First is a thoroughgoing evaluation of archaeological method. The goal of this effort is more than a cataloging of intellectual ills, and, it is hoped, more than a self-serving analysis of bygone days. In the past two decades archaeology has borrowed methods and models from a wide spectrum of physical, social, and historical sciences. In the process a *mélange* of often contradictory assumptions has been unintentionally imported into the discipline. The evaluation of methods focuses on the character of the assumptions and on the problems to which these assumptions tend to direct research. What becomes apparent is the tendency for the assumptions to define problems that are not congruent with archaeology's goal of understanding the long-term patterns of social change.

Second, the present autonomy of methodology is called into question. Middle-range theory, or what we prefer to call method, is both problem and theory dependent. Methods are designed to produce observations that match the expectations our theories generate. Innovative methods are important because they provide new observations and new means of confirming our theoretical expectations. It is important to understand here that the method's conceptual base as well as its units of measure are provided by theory. Stripped of its theory, a method provides measurements, but no meaning. Efforts that stress the development of method as a route toward theory building confuse measurement with understanding.

Finally, we present for debate a problem agenda that once again places archaeology firmly among the social sciences. The problem of understanding the creation, reproduction, and transformation of social organizations remains for us the core issue of the discipline. We may be on firm methodological ground when we discuss changes in lithic technology or shifts in diet; however, these little truths tell us little about changing social relations, and we may be merely tracking the noise in the system. If we ask no

questions about social change, we cannot be wrong: we merely remain confused.

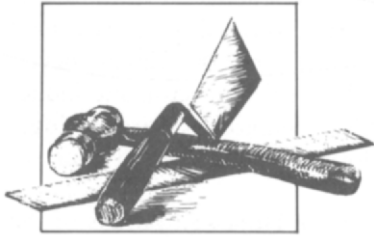
The chapters in this volume fall naturally into two sections. In the first section the products of the archaeological record—the results of our mining of sites—are discussed. Survey sampling, site formation studies, and lithic and ceramic analysis are examined in detail. It is found that many of the methods are applied mechanically, often with the justification that the method merely organizes the data. The issue of justifying the selection of a particular organization of the data seems to be hidden behind a general confusion between hard objects and hard data. In the second section, the precedents of the archaeological record—the concepts of behavior that we bring to the archaeological record—are examined. Here we find that the behavioral concepts implicit in our notions of spatial behavior, decision making, optimization, and population dynamics not only limit the questions we can ask, but subtly guide our selection of theories and problems.

Explicit methods are a hallmark of scientific status, and we do not call into question the importance of rigorous methods. However, the emphasis on methodology has created a confusion between archaeological means and ends. We are confident that this volume will engender debate over the role of methodology in the discipline and hopeful that out of this debate will emerge a new research agenda focusing on the major problems of human social life.

## Acknowledgments

Many of these papers were first presented at the 79th Annual Meeting of the American Anthropological Association in a symposium entitled “Archaeological Hammers and Theories.” We would like to thank the following original participants from that session who were unable to contribute to this collection: C. Wesley Cowan, George Frison, Paul Minnis, Arthur Saxe, and Vincas Steponaitis. Ed Wilmsen was the discussant for that session and subsequently offered several valuable suggestions for the improvement of the volume.

We would also like to thank Sander van der Leeuw, Kate Pfordresher, Maura Keene, and Martin Wobst for advice and support offered during the planning and production of this book. George Armelagos was the first to suggest to us that the Law of the Hammer characterizes much of the trouble with contemporary archaeology. We also thank Albert Spaulding for introducing us to Abraham Kaplan’s *The Conduct of Inquiry*. Special thanks are extended to Academic Press for their patience, and for the support of this project from its inception.



# 1

## Archaeology and the Law of the Hammer

JAMES A. MOORE AND ARTHUR S. KEENE

*Give a small boy a hammer and he will find that everything he encounters needs pounding. It comes as no particular surprise that a scientist formulates problems in a way which requires for their solution just those techniques in which he himself is skilled. . . . The law of the instrument, however, is by no means wholly pernicious in its working. What else is a man to do when he has an idea, Pierce asks, but ride it as hard as he can, and leave it to others to hold it back within proper limits? What is objectionable is not that some techniques are pushed to the utmost, but that others, in consequence, are denied the name of science.*

Kaplan 1964:28

There is a malaise apparent among American archaeologists today—a feeling of uncertainty and indecision about the practice of archaeology, its relevance, and its future. The recent calls for a reassessment of the goals and trajectory of our profession (e.g., Binford and Sabloff 1982; Dunnell 1982; Flannery 1982; Renfrew 1982; Schiffer 1979; Whallon 1982) follow an unprecedented period of growth and innovation within the discipline. Changes within the domains of method, theory, and epistemology were of such great magnitude that the period was regarded by some as revolutionary

(Binford 1968; Martin 1971). It was during this time that archaeology was thought to have begun its transition from a largely descriptive field to one capable of the scientific explanation of social change. The flurry of archaeological activity of the past 15 years has resulted from, among other things, the acceptance of two new research axioms. First, there is much more variability recoverable from the archaeological record than we had previously believed; and second, this variability is the subject matter of general anthropological theory. Spurred by the need to demonstrate that the past is knowable, and, by the belief that new methods give us new means to "observe" the past, archaeologists have pirated methods from the entire range of social and natural sciences. Settlement hierarchies became visible through central-place and gravity-potential models. The organization of subsistence activities was discernible through optimization models. Taphonomic studies sorted out geological, ecological, and chemical patterning in the archaeological record from the patterns created by human behavior. Sampling enhanced our confidence in the representativeness and reliability of our data. Lithic analysis revealed the nature of the interaction of function, style, and cryptocrystalline structure. But something unexpected happened in the process.

In our rush to demonstrate that the past is knowable, little effort was directed toward demonstrating that the past was understandable. Advances enabled us to increase the resolution and the magnification of our image of the past: we were able to reconstruct in greater detail what had transpired and when. However, these advances added little to our understanding of the hows and whys nor do they enhance our ability to understand the long-term transformation of social forms. The recent revolution (cf. Meltzer 1979) in archaeology has not been a paradigmatic revolution, but a methodological one.

## THE LAW OF THE HAMMER

In reviewing the advances within archaeology, the Law of the Hammer comes to mind. Stated in its most elementary form, the law predicts that given a hammer, a young child will find that the world is poundable (Kaplan 1964). The image of the hammer provides a robust metaphor for much of contemporary archaeology. Methods are tools. They are developed for specific tasks to produce specific types of observations. But methods, like tools, can be abused. The most obvious form of abuse involves using methods not because they fit the task at hand, but because they are methods we know and can easily apply. The Law of the Hammer suggests that, although the methods we use are often appropriate to the task, too frequently a given

method is used simply because it is the tool currently in hand. In these cases the pounding has produced more than a little noise, yet little anthropological understanding of prehistory has been constructed (Flannery 1967, 1973, 1982; Dunnell 1982). In the following pages, we examine several methodological innovations to indicate the implications of the dominance of methods in archaeology. We argue that the mechanical application of methods has tended to restrict our field of inquiry and has narrowed the range of questions we are willing and able to ask. Furthermore, tenuous or simplifying assumptions have become imbedded or reified as substantive anthropological knowledge through mechanical application of method and theory.

There is a second way in which the hammer metaphor is appropriate. Tools are in and of themselves often things of beauty—the results of fine and precise craftsmanship. There is also a beauty in archaeological methods—a beauty of logical clarity leading to precise results. There is a danger in this beauty, however: the archaeologist might succumb to avarice and might want to master the method, not for its function, but for its elegance. The recent methodological focus has obscured our vision of the social and historical forces that shape and constrain the work we do. Our aim in this volume is not to catalog either the ills of the discipline or the sins of a generation of archaeologists. More than anyone is willing to admit, contemporary archaeology is ruled by the Law of the Hammer. The subject matter of contemporary archaeology is no longer prehistory, or even anthropological process; today, archaeology is the study of method. This, we believe, is the cause for the general unease gripping the discipline.

## THE TYRANNY OF METHODOLOGY

Why have our efforts in methodology been so unsatisfying? Perhaps, in our rush to capitalize on better ways to observe the past, we lost sight of the reasons for why we study the past in the first place.

We would like to place the recent developments in archaeology into a historical and social context. Our intent is to establish a research agenda for archaeology and to demonstrate the place of methods and models within this agenda. Clearly, archaeology has two unique contributions to make to social science. Its focus on the transformations of social systems over long periods of time emphasizes the interplay of social structure and process over individualizing factors of psychology and will. Additionally, archaeology can document a range of social systems and social relations not manifest in the ethnographic present. Archaeology can, if it fulfills its potential, provide a greater understanding of the full range of social possibilities and lend

insight into the processes that have shaped the modern world. But meeting this challenge necessitates more careful attention to theory, and theory begins with problem selection and careful problem definition. Because of our preoccupation with method, theory (which is informed by our questions) has been severely neglected in contemporary archaeology.

Archaeologists have expended a minimum amount of effort in the development of theories capable of dealing with social systems diachronically. By treating extant stages as extinct ages, disparate ethnographic analogies have been forged into an evolutionary framework (Service 1971). But this is simply an abuse of analogy; ethnology provides us with a largely static analysis of social systems in a modern, capitalist-dominated world economy and gives only weak insights into the diachronic study of society in a preindustrial, preclass world. Relying heavily on ethnography and ethnology for creating our conceptual lenses, we create a situation where we focus on the past, see the present, and remark to ourselves that things haven't changed that much after all. The emphasis on methodological innovation has been so great in recent years that we have failed, generally, to undertake the theoretical and conceptual groundwork necessary to deal with human behavior over large segments of time and space, and within social contexts that predate world systems or social classes.

It is reasonable to argue that we can neither adequately deal with larger questions nor articulate our theory with the real world without developing appropriate methods. Unfortunately, we have come to allow the methods to determine the range of questions we are willing to examine. This phenomenon, which we refer to as the tyranny of methodology, has long been with us in the social sciences. As Mills noted of sociology more than 20 years ago:

Specialists in method tend also to be specialists in one or another species of social philosophy. The important point about them in sociology today is not that they are specialists, but that one of the results of their specialty is to further the process of specialization within the social sciences as a whole. . . . Theirs is not a proposal for any scheme or topical specialization according to "intelligible fields of study" or a conception of problems of social structure. It is a proposed specialization based solely on the use of The Method, regardless of content, problem, or area [1959:59].

When Mills suggested that sociologists caught up in methodology were confusing the methods employed in answering questions for the questions themselves, he could just as easily have been characterizing archaeology.

One of the problems with methodological exactitude is that it can often mask vacuous inquiry and conceptual lethargy. Emphasis on precise method fosters the false belief that all analyses contribute to knowledge, which

consequently congeals to form understanding. Archaeologists often believe that a rigorous, precise, and accurate investigation of a small scale puzzle (e.g., Who made these tools?, What food did these people eat?) is preferable to more loosely constructed investigations of a larger issue (e.g., what factors promote inequality?). Methodological zealotry restricts us to lower level, tractable questions because that is where we have the greatest precision, expertise, and, hence, the greatest confidence. For example, it should not be surprising that recent actualization studies (e.g., Binford 1981) chose hunter-gatherer subsistence as their focus. They emphasize stone and bone physics—possibly one of the least culturally mediated areas open for anthropological study, or, in the very least, a focus that keeps at arm's length economic issues (division of labor, distribution of social production) and political issues (the nature of social power, the processes maintaining egalitarian societies). However, increasing the precision, rigor, and accuracy of our analyses in no way guarantees that the results will be significant. It matters little that we add another decimal place to our measurements: 3.1 is no more meaningful than 3, if we lack the theories that identify such a difference as significant. Problems such as these are not unique to archaeology, nor do archaeologists particularly excel in these abuses. As Mills said of sociology, "Those in the grip of methodological inhibition often refuse to say anything about modern society unless it has been through the fine little mill of The Statistical Ritual. It is usual to say that what they produce is true even if unimportant" (1959:72).

This raises the issue of what constitutes interesting, significant, and important questions. Obviously, not all questions are of equal importance or of equal interest to the anthropological and the larger social science community. Personally, we reject paradigmatic relativism. Ultimately, there must be greater justification for an archaeological endeavor than the claim that the project was doable. The questions we ask and the projects we undertake bear political, moral, and social meaning, whether we willingly shoulder the burden or not. The question of who uses history and to what end is not one that can be dismissed as inappropriate. History and prehistory constitute bodies of knowledge used to legitimize social policies and to validate social trajectories. Certainly the answer to this question affects how we approach prehistory, and how we organize our research. We must therefore gauge the relevance of our questions in terms of how they contribute to our understanding of the social and physical world around us.

We want to emphasize that we are not implying that the development of appropriate methods is unimportant, but rather that methods must be developed with an awareness of context and in response to specific questions. Productive research in archaeology proceeds neither from the top down nor from the bottom up, but through the interaction of data, method,

and theory brought together for the solution of a well-defined problem. It is the question we ask, the phenomenon we desire to understand that dictates the methods, data, and theory that we use. There is a tendency in American archaeology toward least-effort solutions for which appropriate methods have *already* been developed. By letting the methods dictate the questions we ask, we artificially limit our field of inquiry as well as our imagination. It is disturbing that archaeologists find it easier to agree upon what methods they will use in collecting and organizing their observation than upon what questions they wish to solve: we have failed to construct a coherent research agenda (Moore 1980; cf. King 1982; Southwestern Archaeological Research Group 1974). A review of the literature of the past two decades convincingly demonstrates that we have been less than explicit about what we really want to know and why, what we hope to explain, and why it is important. It is toward this end that this volume was assembled.

## PROSPECTUS

Although this book discusses the uses, abuses, and excesses of method and theory in archaeology, it is foremost a book about questions. The authors are explicit about their questions, and the reasons they pursue them. Questions are generated on a variety of scales ranging from the formation and observation of the archaeological record (DeBoer and Wobst, Chapters 2 and 3, respectively) to behavioral issues such as the origins and maintenance of inequality (Moore, Root, and Paynter, Chapters 8, 9, and 11, respectively). Even the dominance of a materialist perspective in archaeology is called into question (Kus, Chapter 12). The authors share an awareness of the need to construct bridging arguments from their particular set of questions to those asked by other anthropologists. All reject, to varying degrees, a narrow particularistic empiricism and a relativism that suggests that all questions merit equal attention.

A number of the authors are sensitive to the role of borrowing in the history of archaeology and its contribution to archaeology's failure to develop a research agenda and an accompanying method and theory of its own. Several authors, critical of the dominance of techno-environmental explanations in archaeology, offer an alternative program that considers social causality and social process. Most of the chapters are also sensitive to epistemological questions. They explicitly recognize that research takes place in a social setting and is conditioned by social and historical circumstances. In many chapters, the emphasis on history promotes a processual analysis of why archaeologists have advocated particular approaches and, in this sense, contributes to an anthropology of contemporary archaeology.

Finally, all chapters reject the pessimism that remains embedded in archaeology and that continues to limit our field of vision. We suggest that the preoccupation with the limitations of archaeological methods and with the poverty of archaeological data has severely circumscribed archaeological investigations. The authors in this volume, through their efforts to trace the route from explicitly delineated questions to methods, expand the scope of our research.

## OBJECTIVES

We feel that it is necessary to reiterate that the objective of this volume is not to chronicle the ills of the discipline. Nor should the reader believe that it is the intent of this volume to deny the importance of methods. Our aim in generating these critiques is foremost to make us conscious of the questions we ask, to consider seriously whether our methods answer these questions, and to remind ourselves that our research is both a result of our social context and a contribution to the creation of new social contexts.

The building of a model, the development of a method, the elaboration of a theory: all these activities are time-consuming, energy-intensive efforts that can be exhausting both physically and emotionally. The investment costs are high, and the attachments we form can be strong. In addition, the rewards for being correct or first are considerably greater than for being wrong or equivocal, unoriginal, or outrageous. Chamberlin's (1965) old and generally ignored warning of the danger of becoming too infatuated or too attached to our pet hypotheses applies as well to methods. The attack, thrust, and parry of methodological battles can be as fierce as any debate over theory. In the heat of discussion, the tactics and strategy of science are often confused. A method easily becomes *the method* and if truth and understanding cannot be staked out, at least ownership of the route to truth can be claimed (Wobst and Keene 1982). Thus, the institutional context in which our research takes place can come to dominate, or at least to obscure, our primary scientific objectives. The constraints of this social context (the need of continued employment, tenure, prestige, competition for grants) are often at odds with the professed goals of social science. This notion is neither new (e.g., Merton 1973) nor particularly remarkable; but it is useful to remind ourselves occasionally that there is not a perfect congruence between the professed ideology of science and its practice, and that the selection of questions, methods, and theories, which is what this book is all about, is not always undertaken while in the state of scientific innocence. The social context of science demands that hammers always be with us, and as Kaplan (1964) notes, the Law of the Hammer is not necessarily wholly

pernicious in its operation—the limits of methods are discovered, the boundaries of theories are demarcated. What are damaging, however, are the attempts to defend the hammer-like uses of one method by attempting to establish it as *the method*.

What is called for, we think, is a good deal of self reflection whenever we undertake research. The chapters that follow exhibit this self-reflection. We warn the reader that the volume does not offer a uniform agenda for archaeological research: the authors do not necessarily agree on the priorities of specific questions, nor are all critical developments in archaeological method and theory covered in these discussions. Other hammers meriting serious discussion include mortuary studies, style, simulation, paleoethnobotany and paleoethnozoology, typology, and statistics. These issues have been discussed elsewhere (e.g., Cowan and Minnis 1980; Saitta 1982; Saxe 1981; Spaulding 1977; Thomas 1978; Vierra and Carlson 1980; Wobst 1980, 1981), and have helped in narrowing the agenda for this volume.

## MOVING BEYOND RHETORIC

Has this not all been said before? Are these same criticisms to be resurrected with each new generation of archaeologists? Certainly the majority of the criticisms we have raised are not original. As the words of James Deetz attest, these are the very concerns that were raised two decades ago at the onset of the methodological revolution.

In conclusion it is perhaps legitimate to ask why we are so concerned with the reconstruction of prehistoric social systems at all. There is always the danger of a certain method or area of inquiry becoming an end unto itself. The true value of such inferences would seem to lie in the direction of the ultimate benefit to general anthropological theory; the elucidation of system and orderly process in culture, past and present. Until and unless this type of inquiry is joined in a systematic fashion to the main body of ethnological theory, the danger is always present of such reconstruction entering the realm of ultimately sterile methodological virtuosity. This should not happen, but it must be kept in mind at all times that such a pursuit must relate in some way or another to the attainment of a broader understanding of culture. There is every reason to be confident that such will take place but, at the same time, the possible pitfalls should be kept in mind [1968:48].

In reviewing the methodological advances in archaeology, we might say that we are now comfortable with the notion that we *can* know the past. It remains for us to define the questions we need to ask, to address why it is

important to ask them, and to develop the integrated programs of theory, method, and data to answer these questions.

## ACKNOWLEDGMENTS

To comment on the state of the discipline takes more than a little nerve. This is especially true in archaeology where such commentaries verge on becoming an overworked and tired genera. To the extent that this overview has avoided that fate we must thank John W. Cole, Warren DeBoer, Bob Paynter, Dean Saitta, Martin Wobst, Maura Keene, Kate Pfordresher, and Bill Fawcett. Not all their advice has been accepted, and they should not be held accountable for our stubbornness. We assume full responsibility for the final content.

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## **PART II**

# **THE PRODUCTS OF THE ARCHAEOLOGICAL RECORD**

Four sets of methods are prominent in our attempts to come to terms with the things we remove from the ground: lithic analysis, ceramic analysis, sampling, and site-formation analysis (i.e., the study of the natural and cultural forces that create the archaeological record). Although these methods are by no means the only important tools used to interpret the products of past human behavior, they are so essential to our work that they have virtually come to define what it means to do archaeology. Regardless of what questions we ask, and what theory we employ, our efforts will ultimately lead us back to the ground and force us to confront the products of the archaeological record. Eventually, we must all come to terms with the two issues emphasized in this section: what are we measuring and how representative are our data.

Not long ago archaeologists abandoned an exclusive concern for time–space systematics with its associated normative concept of culture for a more functionalist–materialist view of the past. In the late 1940s Walter Taylor challenged archaeologists to assign cultural meaning to the objects they recovered—to place archaeological materials within a cultural context.

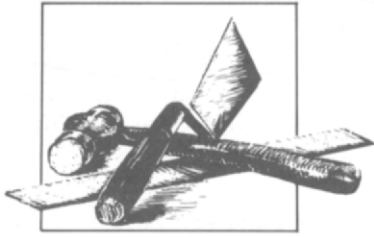
It was not, however, until the 1960s that Lewis Binford and his colleagues demonstrated that this could be done in a vigorous, intersubjective fashion: new measurements, descriptions, and analogies were introduced, enabling us to make statements about archaeological materials that moved beyond the basic classificatory behavior of earlier archaeologists. It became abundantly clear during this time period that artifacts could be described and associations defined in terms of a vast array of measurements and that these measurements could be linked to the cultural context of objects. Measurements of increasingly fine detail were generated. A single stone tool might be characterized in terms of dozens of variables, both quantitative and qualitative, and with considerable precision. In the excitement of improving archaeological analysis it may not have occurred to us that the range of measurements we could generate was virtually unlimited and that frequently we were less than certain of exactly what we were measuring.

The chapters in this part consider this question: what is it we measure in our analyses of the products of human behavior? Generating measurements is not difficult; it is far more challenging to assign meaning to these measurements. But measurements themselves do not create meaning; meaning is imposed on the data and this necessitates establishing the context and the priority of questions at the outset. Because we have the ability to take measurements with nearly infinite detail and variety, we are forced to select those measurements that are more appropriate than others. This task *cannot* be separated from the selection and definition of problems. The mechanical application of methods however, *does* separate these processes. All of the methods discussed in this part have, at times, become mechanical endeavors. This raises an additional set of questions beyond the basic issue of what we are measuring. These questions are: what conditions promote the mechanical use of methods, how does mechanical application become institutionalized within the discipline, what is it we currently need to know about the past, and which measurements are appropriate for the task?

The improved ability of archaeologists to make inferences about the cultural context of objects forced archaeologists to consider another problem—*representativeness*. As many archaeologists abandoned a normative definition of culture, research turned from an exclusive focus on similarity to the study of variability. We began to see past behaviors and their material manifestations in terms of distributions and we wanted to know where observations fit within these distributions. Were our collections typical or atypical? Did our observations capture the mean, the mode, or the tails of the distribution? Representativeness is a thorny issue for archaeologists, given the fragmentary and biased nature of the archaeological record. Greater concern with representativeness forced archaeologists to pay more attention to sampling and archaeological site formation process.

It is commonly accepted that one cannot do archaeology without considering these methods. They are a fundamental part of the archaeological task. But Kaplan's warning of the pernicious effects of the law of the instrument (see Chapter 1) are well worth considering—the dominance of one method often denies others the name of science. In recent years, sampling and the study of archaeological site formation processes have come to dominate the methodological debate in archaeology, so much so that they have become the archaeological agenda for the 1980s. In many cases, investigations into the methodology of representativeness have taken on trajectories of their own, generating tangential studies of finer and finer detail. These investigations seem to move ever farther afield from the original questions these methods were designed to mediate. The gap in the scale of questions we ask is becoming ever greater (e.g., what caused changes in social formations versus what are the earth-moving capabilities of worms) while the links between the two are becoming more and more obscure. We repeat, if our efforts become mechanical they may become meaningless. The danger arises when we remove our methods from their appropriate context, when our methods become ends in themselves, or when we are no longer able to state how our effort fits into a larger design.

Of course, those working on loftier questions may easily lose sight of the significance of fundamental analysis of the archaeological record and of the imperative of coming to terms with our data. The following four chapters place both of these tendencies into context.



## 2

# The Archaeological Record as Preserved Death Assemblage

WARREN R. DEBOER

In a recent book, Woody Allen (1980) suggests that the era of Modern Man begins with Nietzsche's edict that "God is dead" and ends with the Beatles' hit "I Wanna Hold Your Hand." An important and unfulfilled episode in American archaeology can be similarly delimited by the publication of Binford's "Archaeology as Anthropology" in 1962 and Schiffer's "Archaeological Context and Systemic Context" in 1972. Although the temporal span differs by a factor of 10, this wild parallel is perhaps more than cute. Each case begins with grave and courageous intimations of a new era and ends with simultaneously sobering and frivolous afterthoughts.

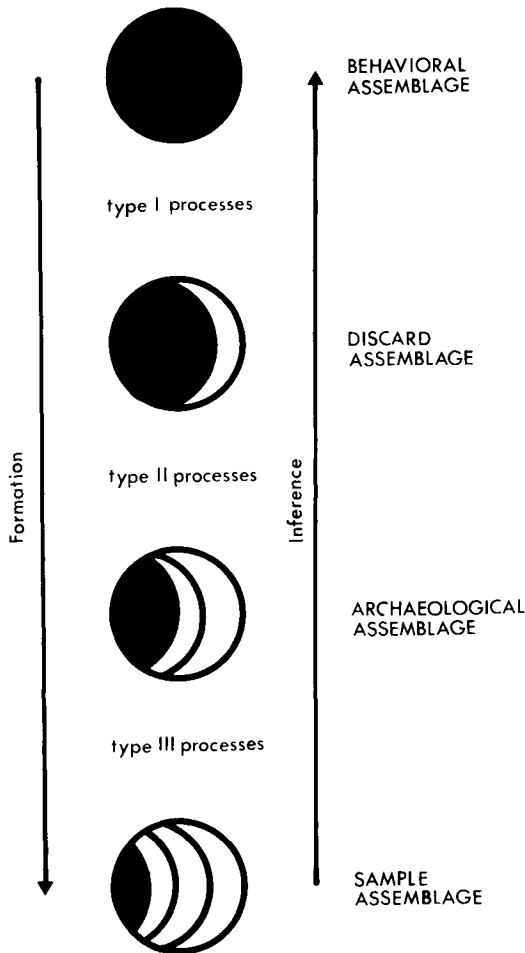
The succession from the "new archaeology," most singularly associated with Binford, to the "behavioral archaeology," most singularly associated with Schiffer (1976), has not been without its critics, both horrified and bemused. In one early revisionist appraisal of the "new archaeology," Flannery (1973) presented his famous characterization of "Mickey Mouse Laws," which make explicit things your grandmother knew but did not tell you because they were so obvious. Still more recently, the laws offered by behavioral archaeology have been likened to a "pachyderm theory"

(Schuyler 1980), which predicts that, other things being equal, an elephant is less likely to slip between a sewer grating than is a penny (a potentially backfired satire that may partly account for why archaeologists have painted such a one-sided picture of Paleo-Indian subsistence).

Despite ridicule, however, behavioral archaeology continues to be an imposing feature on the archaeological landscape and, judging from the publication program of the titular founder of the new archaeology (Binford 1972, 1973, 1977a,b, 1978a,b, 1979, 1980, 1981), it makes a strong claim to be the normal science of contemporary archaeology. Mickey threatens to become a mighty mouse indeed. Here I wish to briefly examine the basis for this intellectual succession and, in a very simplified way, take a look at the accomplishments and limitations of behavioral archaeology.

From its inception, behavioral archaeology has been allied to the general goals of anthropological science, namely, to explain (understand) regularities and differences in cultural behavior. In the case of archaeology, these goals are pursued on the basis of observations made of the archaeological record, which itself consists of the residues of cultural behavior. A cardinal tenet of the new archaeology was that the archaeological record permits inferences to be made about the *total* cultural system which produced it (Binford 1962:219) and that the alleged incompleteness of this record was more a curtailing, if not paralyzing, assumption than an epistemological necessity (Spaulding 1973:338). In practice, rather than in terms of stated general theoretical goals, behavioral archaeology has prospered on the unexamined optimism of this tenet. Its practitioners have emphasized that the archaeological record is a complicated transformation of past behavioral systems, that the nature of this transformation has not been adequately made explicit, and that such explication is a methodological requisite for any reliable inferences relating behavior and its material residues.

In this sense, behavioral archaeology is assuming a position within archaeology directly analogous to that occupied by taphonomy within paleontology, another discipline dealing with "things from the ground." Within paleontology, however, taphonomy has long been a relatively inconspicuous "sleeper" (Efremov 1940). It has become highly visible (Behrensmeyer and Hill 1980) at the same time that paleontology is going through the familiar throes of converting from a largely historical discipline, parasitically subordinate to biology, to a nomothetic discipline with its own specialized body of theory and method (S. J. Gould 1980). Figure 2.1, which will be used in the following discussion, is, in fact, drawn from the paleontological literature (Holtzman 1979; see Clarke 1973 for an archaeological version). I find the constructs of "behavioral assemblage," "discard assemblage," "archaeological (buried) assemblage," and "sample assem-



**FIGURE 2.1.** Schema for the transformation of objects in behavioral assemblages to objects retrieved by archaeologists (sample assemblage). Based on Holtzman (1979:fig. 1) and Villa (1980).

blage” to be a much more useful and adequate portrayal of the complex transformations intervening between living systems and their recovered residues than the simple dichotomy “systemic and archaeological contexts” customarily employed in behavioral archaeology (Schiffer 1972).

In the following chapter, I will concentrate on what, for purposes of simplicity, can be called *type I* and *type III* processes, that is, processes intervening between behavioral and discard assemblages and between archaeological and sample assemblages, respectively. These processes con-

form to the now familiar C-transforms of Schiffer (1976:14–15), that is, *cultural* processes responsible for the formation, destruction, and sampling of the archaeological record. The equally important noncultural processes (N-transforms in the jargon of behavioral archaeology) are not considered here; they receive partial treatment in Wood and Johnson (1978). Furthermore, I have focused at the scale of the site or assemblage and have eschewed the large-scale forces, natural or cultural, which selectively destroy or make less visible sites and assemblages at the regional level (Gifford 1978; Lathrap 1968; Root 1980; Turnbaugh 1978). Finally, I will maintain the important distinction, long recognized and still commonly ignored, between *primary refuse* (where the use and discard loci of objects coincide) and *secondary refuse* (where use and final discard loci differ).

## FOUNDATIONS

The following discussion centers on two archaeologically visible properties of objects: weight and size. Two initial propositions relating weight or size and cultural formation processes are used as a springboard to the large and varied literature of behavioral archaeology. Given the still nascent state of behavioral archaeology, my purpose is more to stress and to illustrate its logic and relevance than to specify a program of how it should or could be operationalized as a routine aspect of archaeological inquiry.

The two elementary properties, weight and size, although not equivalent are interconvertible and have the advantage of being readily measured. The literature of behavioral archaeology contains a large number of propositions relating object weight or size to site formation processes. In fact, it is possible to get a fairly good introduction to behavioral archaeology by examining the logical foundations of the following two propositions, which at face value appear to contradict each other:

1. Light or small objects are more likely to become primary refuse than are heavy or large objects (McKellar 1973; Wall 1973; as cited in Schiffer 1978).
2. As the weight or size of an object increases, one can expect a decrease in the acceptable distance between the object's use and discard loci (see Nettles 1972 for a version of this proposition as applied to the distance between use and repair loci).

As stated, both of these propositions pertain to type I processes having opposite effects. How is it possible that light–small and heavy–large objects are both said to be preferential candidates for primary refuse? To answer this question, each proposition must be examined separately.

What is the basis for proposition 1? Actually there are several independent processes subsumed by this proposition. First, light or small objects are less visible and thus can be discarded less obtrusively. This obvious fact is most operative in cultural contexts where broadcast littering is discouraged. The Wall and McKeller studies deal with Nacirema behavior in the Tucson area, and the same principle is readily illustrated by a study of discard behavior on the Queens College campus in New York City. As shown in Table 2.1, objects with a maximal dimension greater than 5 cm accumulate in trash baskets, while smaller objects litter the rights-of-way of sidewalks. In artifact traps such as hedges (Wilk and Schiffer 1979:333), both small and large objects accumulate in high densities, with a substantial number of the objects probably derived through eolian action. The major pattern, however, is one in which small objects more accurately reflect the actual density of human activity, in this case sidewalk traffic.

A second process embedded in proposition 1 is that light or small objects, *once discarded*, are less likely to be removed in cleanup operations because they are ordinarily less visible. Although here treated as a type I process pertaining to the transfer of objects from primary to secondary refuse, this observation is equally relevant to type III processes. As tabulated in Table 2.2, small objects (in this case, obsidian spalls) are selectively left behind during a quick surface pickup of small fabricated archaeological sites. Many of the techniques of excavation (screening, flotation, etc.), of course, are specifically designed to counteract this size-related effect. At this juncture, it should be noted that size is not the only factor affecting visibility; *salience* (Chilcott and Deetz 1964; R. A. Gould 1980:135) is also important. As shown in Table 2.3, "interesting" objects such as toy soldiers are selectively picked up by casual passersby. Archaeological examples of this process are legion: the crinoid stems from Indian Knoll (Webb 1946:fig. 52), the fluted points from Parrish Village (Webb 1951), and the Hupa-ya *adornos* from Shahuaya (DeBoer 1969) come to mind. I suspect that many of the cases of "aboriginal archaeology" (Schuyler 1968) can be more ade-

TABLE 2.1

Discard Patterns on the Colden Quadrangle, Queens College

Depositional locus	Number of objects	Density of objects (m <sup>2</sup> )	Percentage of large objects
Trash baskets	274 +	—	69
Hedge traps	933	4.94	16
Sidewalks	1196	1.79	3
Lawn interiors	1018	0.37	3

TABLE 2.2

Retrieval of Obsidian Spalls in Successive Quick and Exhaustive Surface Pickups<sup>a</sup>

Extent of pickup	Spalls less than 2.5 cm in maximum dimension		Spalls greater than 2.5 cm in maximum dimension	
	no.	%	no.	%
Recovered during initial quick (90-second) pickup	134	59	94	41
Recovered during exhaustive pickup of remainder	91	82	20	18

<sup>a</sup> Pickups were made from two 4-m<sup>2</sup> plots; the results are pooled. Size measurements were originally made in inches.

quately viewed as the collection of curios than as the utilitarian reuse of ancient artifacts.

Returning to proposition 1, an additional factor is that objects such as small or comminuted bones (Gifford 1980, Gifford and Behrensmeyer 1977; Yellen 1977b), lithic debitage (Stockton 1973), or small sherds are more likely than large objects to be trampled into a soil substrate, although the extent of this process is obviously dependent on the permeability of the substrate and the intensity of trampling. In addition to trampling, mention should be made of *scuffage* (Stockton 1973) which involves the size-sensitive lateral displacement of objects on occupational surfaces and involves what Baker (1978) calls the "size effect." This latter effect entails the upward displacement of large, readily visible objects in a gradually accumulating deposit through selective retrieval and reuse by the human occupants of a site. The "size effect," if shown to be commonly operable, is a happy circumstance as it should reinforce the vertical sorting pattern produced by trampling. Measures of the effects of both processes might well be forth-

TABLE 2.3

Removal of Objects from High and Low Activity Areas, Queens College Campus

Density of human activity	Total objects at beginning	Objects missing after 2 weeks
High	64 potsherds <sup>a</sup> 16 toy soldiers	22 potsherds 14 toy soldiers
Low	64 potsherds 16 toy soldiers	2 potsherds 1 toy soldier

<sup>a</sup> Potsherds are from common flower pots and approximate toy soldiers in size.

coming from the increasing number of lithic conjoinability studies (Bunn *et al* 1980; Cahen *et al.* 1979; Villa 1975–1976).

Although proposition 1 applies primarily to *intentional discard*, it is also relevant, in an oblique way, to *loss*. Simply put, light or small objects are more likely to be lost, although a number of other related factors such as the object's portability and use frequency, as well as the nature of the depositional surface, also condition the probability of loss (Hildebrand 1978; Fehon and Scholtz 1978; Schiffer 1977). The oblique connection to proposition 1 comes about because lost objects, particularly when occupying relatively predictable loci or having great value, are likely to be found (i.e., retrieved from the archaeological record). In contrast, lost objects that occupy unpredictable loci or that are rather expendable, are less likely to be retrieved. Unpredictable loci, almost by definition, are removed from areas where the object is customarily used or stored and thus do not constitute primary refuse in the way this term is conventionally conceived (DeBoer 1976:63; White and Modjeska 1978).

Yet another process that needs to be made explicit in this now lengthy dissection of proposition 1 is that light or small objects are less likely to be obstructions that interfere with future utilization of the discard space. This is clear enough, although an object's potential obstructiveness reflects many factors other than weight or size (e.g., small foot-penetrating flakes or objectionable offal of any kind). The importance of this process will depend on many conditions, including occupational duration (Murray 1980) and the actual and anticipated density and intensity of activity (Binford 1979; R. A. Gould 1980:197). In one of the few detailed applications to actual archaeological evidence, Rick (1980:285–290) attempts to relate size-related displacement phenomena to degree of sedentism and intensity of occupation.

It appears that there are reasonable logical and empirical grounds for accepting proposition 1, although, as has been shown, this proposition breaks down into several components that are always context-specific. This first proposition essentially deals with *thresholds of visibility or obstructiveness* below which objects are likely candidates for primary refuse. In contrast, proposition 2 applies to *inertial thresholds* above which movement costs resist the displacement of objects from use to discard loci. Whether conceived in terms of visibility or obstructiveness (proposition 1) or movement cost (proposition 2), these weight or size thresholds have not been generally defined (if that is indeed feasible), although they have been operationally defined for the purposes of specific studies. One obvious target for the proposition 2 threshold is the reasonable load carried by one individual (analogous to the reasonable daily round-trip distance of catchment analyses); beyond such a threshold there would obviously be strong pressures to move activity to object, particularly when dealing with pedestrian transpor-

tation. But even as trivial as this consideration appears, it is complicated by the fact that the obstructiveness component of proposition 1 counteracts proposition 2. This situation of counteracting forces could certainly be addressed through linear programming (Reidhead 1979) or other techniques designed to specify optimal solutions (in this case, for space management decisions); however, at the present stage, such approaches might just serve to elevate banality, which has the advantage of being quickly recognized, to an obfuscation less easily revealed.

To date, many of the archaeological implications of proposition 2 have dealt with the abandonment of objects as *de facto* refuse or with the converse process of curation. The logic is complicated but proceeds somewhat as follows. The heavier or larger an object, the less portable it will ordinarily be, the less likely it will be moved or curated from its locus of customary use or storage, and the more likely it will be left in place for future use or abandoned as *de facto* refuse. As stated, this is inadequate. Other factors include the remnant use-life of the object, the replacement cost of the object, and the distance between abandoned site and destination. Ebert (1979), for instance, has argued that heavy large stone tools will be curated little or not at all depending on the energetic investment in their production. In cases of anticipated return to a site, the generally large, heavy objects variously called site furniture (Binford 1979:264) or appliances (R. A. Gould 1980:71–72) are either left in place or cached in proximity. As a specific example, 92% of *mongongo* nut-cracking stones, the largest and heaviest abundant objects mapped on abandoned Bushmen campsites, occur within the inner circle of camp refuse, and half of these stones are still associated with *mongongo* nut shells (DeBoer 1978; Yellen 1977a). In contrast, light useful objects with high replacement costs are likely to be curated. Replacement cost as a condition of this proposition can be broken down into procurement cost and manufacturing cost. Emphasizing procurement cost, Gramly (1980) has described the cessation of stone tool curation at quarry sites where raw materials are immediately available. Ammerman and Feldman (1974) and Ammerman *et al.* (1978) have discussed the effect of procurement cost on discard rates and the influence of this effect on the archaeological interpretation of gravity models relating distance and accessibility. With detail that will take a decade to digest, Binford (1978a) suggests that the same general principles influence the consumption, transport, and storage of meat as expressed in the archaeological medium of bones. In the case of the Shipibo–Conibo, sedentary farmers of the Amazonian tropical forest, the somewhat scant evidence for the interhousehold borrowing of ceramic vessels within a single village applies primarily to vessels of low weight (high portability) and low use-frequencies (Table 2.4). In the Shipibo–Conibo example, the ultimate extension of these forces is evinced by tourist wares

**TABLE 2.4**

Number and Percentage of Shipibo–Conibo Vessels of Differing Weights and Use Frequencies Occurring in Households Other Than Those in Which They Were Manufactured

Use-frequency index	High portability (less than 1 kg)		Low portability (more than 1 kg)	
	no.	%	no.	%
Low	7	18	3	8%
High	3	3	1	1%

destined for Lima, Los Angeles, or London; locally available but inferior raw materials are substituted for more exotic but higher quality materials; manufacturing time is minimized; and size is reduced to fit comfortably within a suitcase (Lathrap 1976). My impression of the numerous museum collections of Shipibo–Conibo ceramics is that anthropologists are rarely much different from tourists in this regard, that is, small, portable vessels are selectively curated while bulkier vessels may be represented by a single example. For southwestern archaeologists, this is the familiar “bag the projectile point, just count, perhaps measure, and discard the mano” procedure. In other words, both propositions 1 and 2 apply to type I as well as type III processes.

## EXTENSIONS

In the preceding discussion, I have tried to tease out some of the implications of two propositions relating weight and size to site formation processes of the cultural kind. This discussion has proceeded from relatively simple foundations to increasingly murky and multivariate relationships conditioned by use frequency, replacement cost, and a potentially infinite number of variables which are embedded in the notion “systemic context.” This complexity alone makes it improbable that any archaeological record can actually be “transformed” into (i.e., made isomorphic with) its systemic context. No matter how complicated the transforming algorithm or how elaborated the behavioral chain, such an inductive and reconstructionist approach would seem to be doomed from the start. A more fruitful approach is one in which behavioral archaeology provides a structure of expectation in which the likely archaeological expression of test implications can be phrased. In the remainder of the chapter, I wish to illustrate a number of suggestive propositions relating material properties to behavior, with specific evidence. Thematic emphasis on weight and size, primitive

properties that are archaeologically measurable, will be maintained. The issue addressed is the way by which such behaviorally relevant but ordinarily invisible properties such as use-frequency and life-span may be assessed from the archaeological record.

The data set in question comes from the Shipibo–Conibo Indians of the Peruvian Amazon. The class of objects is ceramic (DeBoer and Lathrap 1979). The observed and estimated values for Shipibo–Conibo ceramic vessel categories that are used in the following analysis are given in Table 2.5.

First we may note that the estimated average weight of Shipibo–Conibo vessel categories displays a weak but significant negative relationship with estimated use frequency (Figure 2.2a). This could be predicted from general least-effort principles, such as Zipf's (1949:73) contention that frequently used tools will be light. A second significant relationship, graphed in Figure 2.2b, is that the heavier (or larger) the vessel, the longer its life-span. The nexus here is less transparent but involves at least two factors. First, since manufacturing time (one measure of replacement cost) is strongly and positively related to vessel weight (Figure 2.2c), heavier vessels are more "valuable" and greater care is likely to be taken in their handling. Secondly, as vessel weight increases, portability decreases, and movement-related opportunities for accidental breakage decrease. A third factor, not considered here, is the allometry of vessel fragility. Finally the above elementary relationships can be combined into composite expressions of strong signifi-

TABLE 2.5

Estimated Physical and Behavioral Properties for Eight Shipibo–Conibo Vessel Forms<sup>a</sup>

Vessel form	Weight (kg)	Log weight (kg)	Estimated manufacturing time index	Log estimated manufacturing time index	Use-frequency index	Median life-span (yr)
Chomo ani	19.95	1.29	29,318	4.42	1	1.13
Chomo anitama	3.88	0.58	7,033	3.84	4	.78
Chomo vacu	0.79	−0.10	2,188	3.34	2	.71
Kenti ani	17.40	1.24	11,600	4.06	1	1.38
Kenti anitama	4.64	0.66	4,124	3.61	7	.88
Kenti vacu	.86	−0.06	950	2.97	1	1.13
Kencha	.50	−0.30	2,475	3.39	14	.31
Kempo	.45	−0.34	1,443	3.15	20	.24

<sup>a</sup> The vessel forms, given their Shipibo–Conibo names, are described in DeBoer and Lathrap (1979). Weight pertains to an average-sized specimen of the vessel form. The manufacturing time index is based on a small number of manufacturing episodes and does not include drying time. The use-frequency index is an approximation based upon field diary listings of use episodes. Median life-span is taken directly from DeBoer and Lathrap (1979).

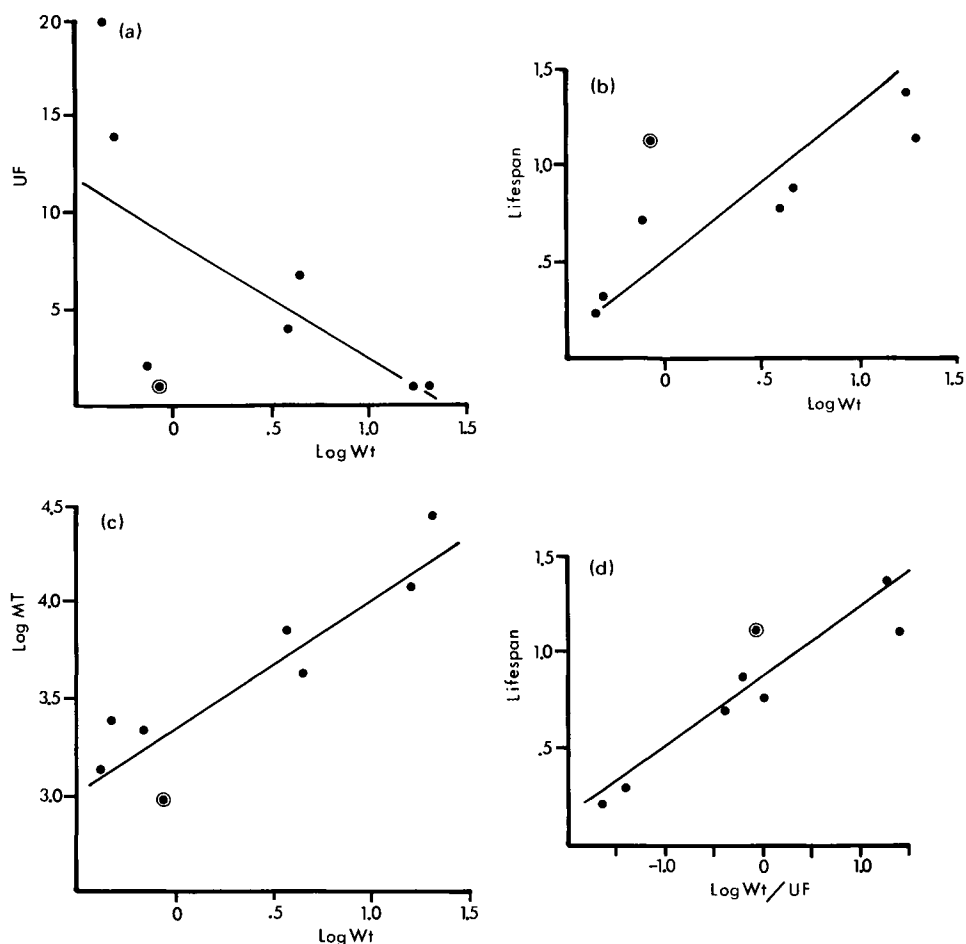


FIGURE 2.2. Graphed relationships for eight classes of Shipibo-Conibo vessels between (a) log weight (kg) and use-frequency index; (b) log weight and median life-span (years); (c) log weight and log manufacturing-time index; (d) log weight and use-frequency index and life-span. Open circles pertain to *kenti vacu*, a frequent outlier in these plots. Use-frequency and manufacturing-time indexes are defined in table 2.5.

cance. For instance, vessel life-span is clearly related to the ratio of weight: use frequency (Figure 2d). Weight alone appears to furnish a useful handle on a number of meaningful behavioral properties which, upon initial consideration, might seem to be archaeologically unknowable. Furthermore these behavioral properties such as use frequency, replacement cost, and life-span, although not directly observable, are precisely the factors that

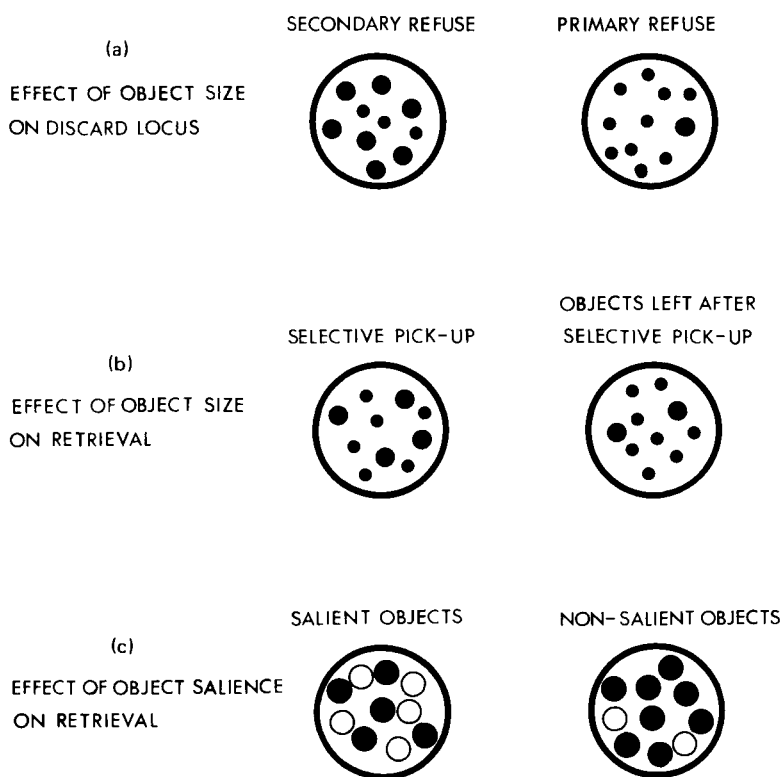
determine what the archaeologist sees in his artifact assemblage, even if he may not know it. For those who argue that the Shipibo–Conibo example deals with the specialized medium of ceramics, a technology based on manufacture through *accretion*, it can be suggested that the same principles apply, *mutatis mutandis*, to *subtractive* technologies such as chipped and ground stone.

## PROGNOSIS

What then can be made of the preceding survey that uses as its documentation cigarette butts and toy soldiers on the not-so-green lawns of Queens College, crinoid fossils from the shell middens of the Green River, *mongongo* nut crackers from the Kalahari, trampled bones from Lake Turkana, exotic lithics from the outback, and beer mugs and food bowls from the Upper Amazon? A scatterbrained eclecticism, perhaps, but all are material expressions of behavioral systems. Furthermore, these are expressions governed by a few definable processes among many which produce the archaeological record.

As commonly phrased, the archaeological record is a static and contemporary by-product of past behavior. This record consists of objects and distributions (Taylor 1948:145). Whether attempting to infer behavior from this record, that is, to “reconstruct” the past, or to use the record as a source of test implications for some model of a past behavioral system, the archaeologist must have some understanding of how behavioral systems and their residues are interrelated. Without such understanding, the archaeologist has no way of knowing what his observations signify. More bluntly, he does not know what he is doing. In this context, behavioral archaeology *is* archaeological methodology (or, if preferred, “middle range theory”) linking observation and theory.

Such a strong claim may seem preposterous, particularly in light of the simple and reductionist examples provided in the preceding pages. But let us take another look at the three experiments performed to exemplify proposition 1 (Figure 2.3). I suggest that, when shown in this illustrated form, the differences between the left-hand and right-hand circles are stark enough to indicate that small-scale, “rinky-dink” experiments of this kind reveal differences that rival those to which archaeologists, often with the aid of more high-powered approaches, routinely ascribe significance, whether at the level of activity areas or occasionally even at the level of entire cultural traditions. Thus, it is easy to argue that a codified set of notions concerning



**FIGURE 2.3.** Illustrated summary of the experiments tabulated in Tables 2.1–2.3: (a) the effect of object size on discard locus (large objects, ●; small objects, •); (b) the effect of object size on retrieval; (c) the effect of object salience on retrieval (retrieved objects, ○; unretrieved objects, ●). Each circle represents 10% of the total.

behavioral properties and their effects on the rate and locus of object discard is essential if behavioral meaning is to be attached to the archaeological record. The fact that so much meaning has been found in the archaeological record without recourse to such notions, either implicitly or explicitly, is not surprising and probably indicates nothing other than that some semblance of order, whether imposed or derived, is necessary for anything the human mind is to comprehend (Arnheim 1971:1).

Of course, this does not mean that the study of site formation processes and behavioral–material correlates is the top priority within archaeology. Nor does it mean that behavioral archaeology can ever be a perfect negen-

tropy machine, proceeding up the inferential ladder from sample assemblage to behavioral assemblage, and eventually transforming fugitive objects and their provenience into a totally reconstituted behavioral system. As suggested earlier, such a goal is illusory and backward: all archaeologists should know by now that a time-machine would more complicate than facilitate their theoretical goals. The proper role of behavioral archaeology is more modest, namely to translate the expectations of models of any theoretical ilk into archaeologically visible and viable units and to point out that objects within behavioral systems do not simply precipitate into the ground.

Given all the theoretical activity and hammer-like application of techniques that have been signs of an exuberant and healthy archaeology during recent times, behavioral archaeology—designed by archaeologists for the peculiar realities of the archaeological record—has, I suspect, an important and permanent place.

## ACKNOWLEDGMENTS

Many people helped; only a few can be mentioned. David Wilcox offered a punishing but extremely useful critique that influenced me more than it did the final version of this article. Michael Schiffer was encouraging over a period of years and furnished a number of important bibliographical leads. Sara Stinson enabled me to finish by listening and laughing. To all, I am grateful. As is customary, I am responsible for the outcome.

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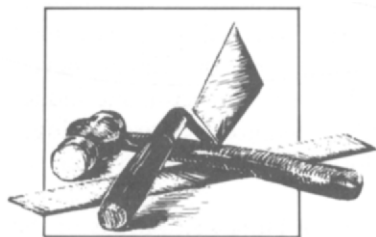
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# 3

## We Can't See the Forest for the Trees: Sampling and the Shapes of Archaeological Distributions

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Our discipline is infected quite frequently by new methods that promise quick and simple answers if only we had the right questions. Particularly susceptible to such infection are mechanical endeavors—those aspects of problem solving that are taken to be mere stepping stones to the “real” questions of the day, but are not likely to involve investigators in confounding basic assumptions. In the archaeological literature of the 1970s, an interesting example of such methodological fervor is provided by sampling, a method that pervaded the study of archaeological distributions far more severely than previous methodological pandemics. In combination, this one method (sampling) and this one task (specifying archaeological distributions) account for the lion's share of archaeological fieldwork. Their epidemiological relationship is the focus of this chapter.

To increase resolution, I first lay out some of my basic assumptions about the archaeological record. These assumptions provide a yardstick for chronicling the evolution of that part of theory and method that helps

specify archaeological distributions. What I say about the evolving state of the art would be irrelevant, however, if I did not relate it back to archaeological practice. Toward this end, I take a detailed look at a spatially and temporally defined population of archaeologists, to see what rationales and methods are actually put to work. My critical evaluation of theory and practice, offered by way of conclusions, is quite specific to the relationship between archaeological distributions and the ways we observe them. Nevertheless, I hope that my case study will bestow some generic immunity against methodological fervor wherever it may arise. I should also make it clear that I am primarily concerned with distributions that extend beyond the confines of the individual "site."

Ideally, I would like this chapter to be read by (1) archaeologists interested in cultural resource management, (2) archaeologists who believe that distributions must be fully known before any real behavioral questions can be asked, and (3) all those practitioners whose problem finds its expected resolution in some aspect of the archaeological record that is spatially distributed. This target audience shares the perceived need to specify archaeological distributions in space. Beyond that, to the extent that this chapter addresses a question of method and grounds it in theory, other archaeologists might benefit from its morals.

## THE SCIENTIFIC TASK

Before delineating scientific tasks, I need to set down some basic assumptions about the phenomenon in which my target audience is interested, that is, about the shape of archaeological distributions. The archaeological record is assumed to consist of the material precedents and products of behavior. Obviously, if our research universe is defined in this way, there is bound to be a rather intimate articulation between human behavior (the variable about which we want to make statements), those variables that facilitate it and constrain it (the material precedents), and those variables that are materially affected by it (the products). The variance in the distribution of material products and precedents in space and time should inform us about human behavior in ways which differ from the observation of behavior itself. Thus, it should provide archaeology with a data base which is relatively unique, yet important within the behavioral sciences.

Variable by variable, and *in toto*, I assume that these material products and precedents approximate a continuous distribution in the three dimensions of time and space. This has some good behavioral rationale. For example, there can be little squabble with the notion that human behavior is temporally continuous. At the same time, at least in the recent historic and

prehistoric past, no parts of the landscape have been categorically inaccessible to humans. This does not imply that their behavior should be uniformly distributed. It is bound to be strongly clustered, counterposing areas of extreme density with extremes of low density, minimal clustering, and maximal dispersion. Yet, the low-intensity tails of this distribution are infinite rather than neatly bounded, and peaks are not separated from each other by a vacuum, but by a continuum of behavioral space.

What is true of human behavior should apply as well if not better to the distribution of its products and precedents. At a given time, the interaction of a given population with a set of nonhuman and human “environmental” variables may well be confined to a finite number of locales. But this should not be true of the material nodes of this interaction. Many precedents, for example, land surface, climate, biomass, population densities, fauna, flora, and soils, are vastly more continuous in space than the human behaviors with which they articulate. The same is true of the material products of the interaction. Whether one looks at the behavior, genetics, or demography of living populations, at the human impact upon nonliving variables such as the climate or the land surface, or at artifacts as archaeologically most graspable products of behavior—through time, their distribution should acquire an infinite tail. Clearly, different kinds of products and precedents will have different degrees of clustering and dispersal in time and space. Yet, no part of the countryside is categorically devoid of any material input to, or output from, human behavior.

In the context of my basic assumptions, my target audience faces similar scientific tasks. They revolve around the question, What is the shape of the distribution of the archaeological record (or some selected aspect of it) within a given area (and time period)? An archaeological distribution is best visualized as a blanket that covers the given area. Corresponding to changing observations per unit space or volume, this mathematical construct will have peaks, valleys, and other “topographic” features much like a real three-dimensional landscape. A given distribution can be said to be fully specified, if our model of it satisfactorily retrodicts our observations and concisely anticipates those possible observations that have not yet been collected.

Short of actually gathering all possible observations, four different scientific tasks help us in developing satisfactory approximations of distributional shape: prospecting, sampling, estimation, and hypothesis testing. Let us briefly differentiate them from each other. *Prospecting* implies an informed search for qualitatively unique subsets of observations (e.g., locating all “sites,” or finding all high counts). *Sampling* in a broad sense simply refers to all those methods that select subsets of observations from the theoretically possible ones. Defined in this way, prospecting would be en-

compassed by sampling. More narrowly circumscribed, sampling does become different from prospecting, however. That is, it may refer to those kinds of methods that select subsets of observations to make statements about all theoretically possible observations (e.g., observing 10% of all spatial units to characterize all of the units). *Estimation* methods allow us to evaluate how well our observational subset characterizes the population as a whole, or to specify the kinds of population values of which our observations could be a subset (e.g., estimating the total number of artifacts in our area, given our sample of observations). *Hypothesis testing* circumscribes the logical operations by which we can evaluate our distributional models against relevant observations (e.g., assuming that we are dealing with hunter-gatherers in a temperate forest landscape, how should their material precedents and products be distributed across the given area?). There is no necessary rank order or sequence in these scientific tasks if our goal is the study of archaeological distributions. At the same time, the elegance with which we complete these scientific tasks does not depend only upon our familiarity with the finer points of method. At least as important is our familiarity with anthropological theory and preexisting knowledge in general, and with its implications for the given archaeological record specifically.

## THE EVOLUTION OF THE METHODOLOGICAL STATE OF THE ART

Since preexisting knowledge and anthropological and other relevant theory are unbounded, we do not gain by chronicling their evolution. A much more modest review should suffice here, focused closely upon the archaeological distributions themselves. Our ability to interact scientifically with these distributions depends upon the state of the art in research on their shape and on their *generating functions*, on the state of methodology in prospecting, sampling, and estimation, and on our ability to ask intelligent questions about them. *Generating functions* are those categories of knowledge that help to approximate archaeological distributions in space, by translating knowledge about the distribution of human behavior into forceful expectations for the distribution of its products and precedents. My historical review focuses on these topics as they become visible in the nationally exposed literature, rather than in the dependent clauses and appendixes of local and regional publications.

For ease of presentation, I break up the flow of time into four periods: pre-1960, 1960–1969, 1970–1975, and 1976 to the present. The boundaries between these periods, while arbitrary, relate to the publication of important new information about our topic.

## The Pre-1960 Background

The first 100 years of archaeological research contributed relatively little of lasting value about the nature of archaeological distributions. The predominant culture-historical orientation, with its principal tools of typology and stratigraphy, invited archaeologists to pursue points at the expense of distributions, to treat distributions as unbounded and discontinuous, and to focus upon a select set of artifact classes and field contexts to the virtual exclusion of others (cf. Willey and Sabloff 1980). In this matrix, distribution studies were needed (1) to bound archaeological entities in space (“cultures”) and (2) to establish temporal sequence in these entities (“history”).

If the distribution of cultures was the goal, this goal was best reached by selecting the most variable class of material culture for study—usually projectile points or pottery. Highly variable artifact classes made it easier to produce “types,” that is, sorting categories that minimized intragroup variances and maximized intergroup variances. By mapping all occurrences of a given type, one could not fail to obtain externally bounded, yet internally discontinuous mapping entities (“cultures”). Under the paradigm of the time, such procedures were fully consistent and, thus, virtually immune to falsification. All one could do was to improve upon the finer points of the maps: (1) by searching the bounded entity for additional points of occurrence of the given type, (2) by searching the extant points for additional types or traits with distributions congruent with the given type, and (3) by mapping similarly conceived, adjacent entities.

In hindsight, distributions were shortchanged by a methodology that explicitly concentrated on points, on artifacts removed from their context, and on units bounded with formal criteria whose relationship to behavioral variables was implicit at best. While the procedure elicited some information on spatial variation, it could not accomplish this very elegantly. After all, it explicitly selected what was formally alike from a class of material culture which had itself been selected because it was so pleasingly variable. Moreover, the resulting distributions remained two-dimensional, entirely lacking in shape within their bounds (since a single presence added a point to one’s map as successfully as a multiple one).

A methodology that is essentially context-free and inductive is not the most sensitive tool for contributing information about the generating functions of archaeological distributions. If the typological method could be applied to any collection of spatial data, one did not need to concern oneself with sampling and estimation. On the other hand, the search for “cultures” was best accomplished if one prospected for large or dense sites, that is, sites with disproportionate chances for scoring a typological presence or association.

Once horizontal taxonomies had been achieved, stratigraphy could be employed to provide vertical order and, thus, history for the cultures. Stratigraphy required a search for superposition, again most likely available at the largest sites: The optimal solution to a culture-historical puzzle would be a single point where all of a region's typological complexes could be found on top of each other, separated by sterile deposits. Although stratigraphy clearly added a third dimension to distributional studies, this dimension was as discontinuous as the two horizontal ones. It did not raise the sensitivity of archaeologists to the three-dimensional, highly variable and continuous nature of those processes that generate archaeological distributions. Instead, it worked together with the large site bias of typology, diverted attention from issues of sampling and estimation and, for all practical purposes, constituted a prospecting method for those few and unusual points where superposition of typological difference was maximized.

Seriation had a much more beneficial impact on distributional know-how than either typology or stratigraphy (Brainerd 1951a; Robinson 1951). Seriation works better when there are more sites within short distances, and when these sites reflect relatively short occupations. Seriation necessitated prospecting on a denser grid and for more ephemeral and surface sites, thus producing denser and more contiguous site distributions. Also, the accuracy of seriation increases with more types or attributes and with more representative collections from a given site. This necessarily increased the concern for intrasite distributions, raised questions about the appropriateness of various sampling methods (Spaulding 1953a), and stimulated work in archaeostatistics well beyond the original histograms on which the method depended (e.g., Spaulding 1953b). Finally, counts, histograms, and percentages demonstrated that archaeological distributions could be treated as continuous in space and time.

A number of prospecting tools developed during this period, almost incidentally. They included soil phosphate analysis to delimit areas of intense prehistoric land use (Arrhenius 1931), resistivity survey (Atkinson 1952) and magnetic prospecting (Aitken 1958) in pursuit of buried features, and photography to identify sites from the air (e.g., Willey 1953). Given the orientation of the field, however, none of these methods found wide application. Sampling methods were consistently applied only to shell mounds, primarily in California where column samples to estimate total shell content were systematically evaluated (e.g., Cook and Heizer 1951; Treganza and Cook 1948). Other research on sampling, probability, and statistics by and for archaeologists did not concern itself with archaeological distributions, but exclusively with typological questions (e.g., Brainerd 1951b; Kroeber 1940; Spaulding 1953b).

The 1940s and 1950s did see the completion of a number of large-scale

regional surveys in areas of high archaeological visibility—the Lower Mississippi (Phillips *et al.* 1951) and Peru (Ford and Willey 1949; Willey 1953)—in pursuit of culture-historical goals. Yet, if these surveys are evaluated with the hindsight of the 1980s, it is clear that they suffered from a large-site bias. They failed to provide data representative of the sampled distributions, and they did little to advance archaeological methods for sampling and prospecting. Moreover, lacking an explicit rationale for excavation and survey, archaeologists were unable to evaluate the accuracy, precision, and consistency of their results. In fact, so serious were the shortcomings of pre-1960 archaeology that the methodological advances of the 1960s and 1970s consist primarily of programmatic statements in response to previous failures, rather than of well-justified applications to resolve important research questions.

### 1960–1970

One such programmatic article initiated the new decade (Vescelius 1960). It set out in clear, if highly technical language, that all archaeology is sampling; and that this generalization could be turned to advantage if archaeologists familiarized themselves with sampling as a formalized mathematical methodology. Sampling theory offered a number of explicit avenues that moved beyond descriptions of samples to statements about the population from which they were drawn—to estimate population characteristics accurately and precisely (as long as the sample was properly selected) and to evaluate the accuracy and precision of these estimates. Vescelius illustrated his point by using a cluster sample drawn at random from the spatial units of a site to provide and evaluate estimates of pottery type proportions. The method formally invited statements about populations and distributions from a limited number of observations. More importantly, if sampling technique was properly applied, one could formally evaluate these statements for their accuracy and precision. In short, although mathematically tedious and demanding in rigor, the method added significantly to our understanding of archaeological distributions. Nevertheless, probably because he introduced a method without identifying interesting problems to which it could be applied, Vescelius' paper never received more than token citations.

This changed with Binford's (1964) publication of "A Consideration of Archaeological Research Design," an article that expanded methodological expertise in a number of different dimensions. Because sampling was fully integrated into anthropological problem solving with an explicitly defined and justified research design, Binford's paper became the model for archaeologists sensitive to considerations of sampling. In the first place, the article

categorically refocused attention from collections of artifacts or sites to the regional cultural system and from a preoccupation with culture history toward questions of process and adaptation (cf. Binford 1962). Such a focus required far more exacting knowledge of archaeological distributions than had been needed heretofore. Given our inability to x-ray an entire region, this knowledge could only be acquired by a multistage research design, regional in scope, which employed sampling logic at every stage of its execution. In other words, statistical sampling had become an integral part of the research design and agenda of processual archaeology. Distributions of cultural items, cultural features, "cultural activity loci," and ecofacts had become the central data base.

In addition, the article laid down the terminology and basic logic of statistical sampling in systematic fashion. Although it was short on math and problem-solving examples, it provided excellent advice on how to select one's sampling design for the given stage of one's research. Specifically, the article introduced archaeologists to stratified and systematic cluster sampling, in addition to the simple random cluster sampling of Vescelius; it discussed the problems of sample size, sample fraction, and sample units in great detail; and it stressed repeatedly that the most auspicious applications of sampling would be those in which the particular sampling scheme was optimally adjusted to, and justified within, preexisting knowledge and research goals.

The other papers on sampling during this time period did not add much to the scope of Binford's article. For example, in a case of independent invention, Rootenberg (1964), in an article short on math and with some marginal violations of sampling logic, spelled out a sampling design for stratified random cluster sampling. He failed to mention that this had been one of the stages in Binford's agenda. That same year, Cowgill (1964) provided a programmatic introduction to random element sampling. Although he applied the method to extant sherd collections, it could also be called into service for spatial sampling of elements. Ascher and Ascher (1965) must be given credit for the first hypothesis test involving archaeological distributions when they evaluated claims of early human artifacts from the Americas against a set of systematic cluster samples. Yet, by the end of the decade, the first generation of Binford's students at the University of Chicago and many of the contributors to *New Perspectives in Archaeology* (Binford and Binford 1968) had already applied sampling to solve spatial problems (among others, Hill 1967; F. Plog 1968; Struever 1968; Whallon and Kantman 1969).

It is worth noting that *American Antiquity*, after 1964, published only one programmatic article on regional survey that did not concern itself with probabilistic sampling (Ruppé 1966). By 1967 the new topic had become

sufficiently normal that even a relatively staid guide to field method had jumped on the bandwagon and added a paper on sampling archaeological distributions (Ragir 1967). This particular article provided an introduction to the mechanics of sampling in archaeology, rather than to the research design that Binford's (1964) article treated. While it addressed basic assumptions and statistical logic in more detail, it did not materially alter Binford's advice, except perhaps in noting that systematic sampling should be of limited utility since it would not permit the calculation of confidence intervals around one's population estimates.

By 1964 all had been said to stimulate thought about the shape of archaeological distributions, namely, (1) problem solving usually requires archaeological *distributions* at the site and regional level (Adams 1968; Binford 1964; Struever 1968); (2) knowledge of these distributions derives from small samples; but (3) if we acquire these samples properly, we can make accurate and precise statements about the distributions in their entirety. If we only were to take these messages to heart, we should expect information about distributional shape to emerge. Unfortunately, by the end of the decade, this promise had remained unfulfilled for reasons that became clearer later.

The program had been diverted from estimating and specifying distributions to providing representative and unbiased samples. The samples, in turn, were treated as populations in their own right, answering quite different questions. In other words, the distribution was assumed to be appropriate for the application of sampling methodology, and it was sampled to obtain a "representative" sample. Rather than serving to specify the distribution in more detail, this sample was turned into the new research population against which answers to interesting questions could then be evaluated. Such a strategy could not have added much of interest regarding the shape of the sampled distributions, and even the representativeness of the samples had to remain in doubt.

Similarly, the 1960s provided only incipient information about the generating functions of archaeological distributions. Sampling, as advocated by Binford, Ragir, Rootenberg, and others, required at least a kernel of knowledge about variables that may influence the distribution of human behavior across the landscape. But since this predictive information was merely consumed in designing strata rather than evaluated by the research design, it may have resulted in predictive confidence where none was warranted. The possibility remained that other partitions of the sampled distributions, including random ones, would have yielded differences between the strata as large as or larger than the observed ones.

The only explicit attempt to evaluate the generating functions of a regional archaeological distribution was Thomas's (1969, 1973) Reese

River valley survey. Using variables derived from Steward's ethnography of the Shoshone as a point of departure, Thomas estimated their artifact-generating potential in space and time. A somewhat underspecified computer simulation helped to produce the expected archaeological distribution across the survey area, assuming Steward had been correct. Contrasting the expected with the observed distribution, Thomas concluded that Steward's ethnography was a reasonable predictor of the archaeological record.

Three points need to be raised about Thomas's research design: (1) It was the first formal test of a generating function for an archaeological distribution. Since the specific function (the ethnohistoric reconstruction) could not be falsified in any major way, it stimulated archaeologists elsewhere to sift through local ethnographies and ethnohistories for their value in predicting archaeological distributions. (2) If a given generating function cannot be rejected by archaeological data, it has not yet been established as the best or only one to generate the observed distribution. (3) The use of *sampling* to evaluate the fit between predicted distributions and archaeological ones is spurious and lies outside the logic of Thomas's research design, a point which was not publically addressed until the end of our next time period (see the following discussion of Cowgill 1975).

Other contributions to our topic during this decade were either restricted to regional and local publications, or they were concerned with prospecting method. Only a few of the latter ones have applications outside of the desert areas of the Southwest. It might be of interest that this period saw the first American use of proton magnetometry—for identifying buried palisades (Black and Johnston 1962; Johnston 1961) or to locate hearths and firepits prior to excavation (Ezell *et al.* 1966). Core drilling of archaeological sites was practiced systematically in a few contexts (Price *et al.* 1964; Reed *et al.* 1968), and aerial photography was successfully applied to locate prehistoric and historic fishtraps on the east coast (Strindberg and Tomlinson 1969).

## 1970–1975

This time slice begins with the republication of *Hatchery West* (Binford *et al.* 1970) and concludes with the first of a number of symposia on archaeological sampling, called together to digest the explosively expanding literature on this topic (Mueller 1975).

*Hatchery West* was one of several monographs and papers that concerned itself with a problem of considerable interest in dealing with those parts of the country where only parts of the archaeological record expose themselves at the surface. To what extent does the archaeological distribu-

tion on an intensively collected surface correlate with or predict the *in situ* subsurface distribution of archaeological material (see Redman and Watson 1970; Schiffer and Rathje 1973; Tolstoy and Fish 1975, and others)? If the areas were freshly plowed and had received recent rainfall, the coincidence of both distributions was found to be considerable to depths of approximately 50 cm, although specific behavioral hypotheses frequently needed to be employed to link the surface with the subsurface (e.g., sherds are not discarded on house floors but in middens, and preceramic cooking areas should be rich in fire-cracked rocks). Equally important was the conclusion that, beyond that depth, nothing short of excavation would reveal the shape of the subsurface distribution.

In another important paper at the beginning of the period, Cowgill (1970) reminded archaeologists that they were logically dealing not just with one kind of distribution but with three different ones—physical events (the actual behaviors), physical consequences (the consequences of such behaviors), and physical finds populations (what remains of these consequences to be found today). While sampling and various prospecting methods provide information about the distribution of physical finds so that its shape may be specified, this distribution is usually a very biased sample of the physical consequences distribution which in turn is usually a rather distorted sample of the physical events distribution. Statistical sampling provides a means of making estimates about the physical finds distribution. It is not a methodology to estimate either of the other two distributions. The extent to which physical finds distributions are representative of the others needs to be established by hypothesis testing. This is particularly germane to those areas where the archaeological record is poorly preserved and poorly visible (cf. Collins 1975, and various papers by Schiffer).

Meanwhile, Binford's (1964) research design was being consumed in an ever-increasing number of regions, predominantly where there was excellent archaeological visibility on the surface. These applications were quite frequently accompanied by attempts to evaluate the utility of sampling for the estimation and specification of archaeological distributions. Examples include sample surveys in the Rio Grande Valley (Judge 1973; Judge *et al.* 1975), Elk Ridge (DeBloois 1975), Cedar Mesa (Matson and Lipe 1975), central Arizona (Mueller 1974), Cache River, where only plowed fields were surveyed (Schiffer and House 1975), Reese River (Thomas 1972, 1973) and others. These surveys shared a number of features: (1) restricted time, money, and personnel made sampling unavoidable; (2) sampling designs were stratified according to the judicious use of prior knowledge about regional environments and cultural behaviors; (3) there was usually some sort of multistage survey strategy in which limited sampling was carried out to get a rough idea about the sampled area; followed by (4) a set of in-

creasingly fine-tuned objectives and associated research designs. While this procedure had already been recommended by Binford (1964), it became further dogmatized in a number of "cookbook" publications, including those of Peters (1970), Redman (1973, 1974), Jelks (1975), and Read (1975). During this period, only a few of the samplers directed their attention to the shape of distributions within clusters of archaeological resources ("sites").

What did we learn from this veritable explosion in sampling applications? As before, relatively little. There are several reasons for this. Principal among them is the problem that sampling was applied primarily because next to nothing was known about the shape of archaeological distributions in the given areas prior to the beginning of the given survey. Sampling was applied to provide a representative sample from the distributions. Yet, once the sample had been collected, it was usually employed for far more mundane purposes than to further specify the distribution, to investigate whether the sample was actually representative of the population, or to see what variables actually determined distributional shapes in the given case. In the best of circumstances, the sample was actually used to estimate certain overall population characteristics, and to establish confidence intervals around these estimates. Usually, however, as before, the "representative" sample was taken to serve as a new research universe which thereafter could be utilized to answer all sorts of questions, with none of the answers particularly informative about the shape of the distribution or about its generating functions.

Unfortunately, the same criticism applies to the large number of projects in which archaeologists carried out fieldwork to test whether sampling actually would provide precise answers about the shape of archaeological populations and to determine which of the many alternative sampling designs would produce the most accurate information about population characteristics (Chenhall 1971; Judge *et al.* 1975; Lipe and Matson 1971; Matson and Lipe 1975; Mueller 1974). As a rule, such studies used an area that had been completely surveyed. They then generated a number of random samples under different sampling designs, and evaluated how well the sample of observations predicted a number of population characteristics. For a number of reasons, which became sharply focused only much later (see also Hole 1980), not much could be learned from these projects about "optimal" sampling procedures or about the utility of sampling in archaeology altogether.

If the sampled population is completely known, statistical theory can provide *exact* answers about what should be the most appropriate sampling design for estimation purposes. The method which gave the best estimate of population characteristics in one case was not necessarily the best in other

cases, given the variation in archaeological distributions in space. Since sampling is usually applied where the shape of distributions is only imperfectly known or can only be imperfectly anticipated, the use of area-specific optimal designs elsewhere is bound to increase the noise in the estimation procedure. Moreover, the optimality of estimates has its own probability distribution. Because its robustness can only be evaluated with a very large number of trials, the simulators with a small number of trials were bound to produce a number of spurious if not contradictory recommendations.

In hindsight, the time spent in evaluating the comparative merits of competing sampling designs could have been spent more productively. For example, the shape of the perfectly known distributions could have been accurately specified. One could have specified which aspects of the archaeological record have what kind of distribution, or one could have investigated their generating functions. In fairness, it should be pointed out that the sampling cookbooks during this 5-year period were quite insistent that there was no ideal sampling strategy. Instead, optimal sampling application required a judicious combination of archaeological and other knowledge pertinent to the project and its scope, finances, time, and personnel, as well as the desired levels of accuracy and precision for the sample-based estimates.

Few contributions went beyond what was already known at the beginning of this period. A noticeable exception is a well-reasoned comment by Cowgill (1975), in which he carefully differentiates sampling from selection (hypothesis testing in our definition). Selection is called for when explicit hypotheses exist about relationships among variables expressed in distributions. If one wants to evaluate these hypotheses, criteria of relevance need to be defined. These criteria should guide the investigator toward the necessary and sufficient number of intrinsically important observations, that is, to purposefully pick relevant observations. In terms of specifying archaeological distributions, this rationale should be superior to and less wasteful than sampling, to the extent that there are explicit hypotheses about the shapes of distributions. Since the model is evaluated logically, it should require less fieldwork than sampling to acquire the same information about distributional shape. In other words, it should take fewer observations to test a model than to build it up from observations. Another advantage of the selection approach over sampling lies in the fact that, in a single research design, we will learn something not only about the shape of distributions, but also about their generating functions. That is, selection provides evaluated hypotheses, whereas sampling generates untested estimates.

A number of papers during this 5-year period provided some incipient insights into the generating functions of archaeological distributions, beyond the ethnographically derived ones of Thomas. For example, Vita-Finzi and Higgs (1970) introduced the method of site-catchment analysis as a

predictive tool, on the basis of locational geography and hunter–gatherer ethnography. Given distance decay, people should exploit only those resources from a given settlement which are within a given radius around it. Thus, given the settlement, one should be able to predict from its catchment what activities were carried out within it. Alternatively, given this radius and the resources required by settlement populations, one could predict where settlements should be located so that access to the requisite resources is guaranteed. An article by Wood (1971) introduced a number of additional models for predicting distributional shapes or for comparing observations of distributions with distributions produced by known generating functions, including Hudson's location model,  $n$ -dimensional niche space analysis, as well as Poisson, negative binomial, Neyman's type A, and Dacey's regular Poisson distributions.

An explicit test of the utility of these approaches was carried out by Williams *et al.* (1973) on a version of the  $n$ -dimensional niche space model. In this test, seven criteria derived from regional ethnography and general considerations were determined to be important for the location of sites. In the field test, a site was expected where at least five of these criteria co-occurred. This prediction proved to be accurate in 95 of 100 cases. In other words, the method allowed adequate specification of archaeological distributions prior to fieldwork. Archaeological fieldwork could then be used to evaluate the accuracy of the model and to specify it in greater detail, while contributing simultaneously some information about the driving variables behind such distributions.

Another aspect of archaeological distributions entered the archaeological field of vision only very gradually—the low-density dispersed parts of the three-dimensional continuum. As sampling increased in intensity and decreased in grain size, it became more obvious that sites were usually embedded in dispersed artifact distributions with apparently infinite tails. Although this observation was repeated in many regional surveys, it was conveniently overlooked when specifying a given project's relevant distributions. There simply was no good rationale for integrating this material with data generated from sites. To collect such observations more systematically would have been too labor intensive, and nonsite materials were significantly less glamorous than those from sites.

Several people independently attempted to develop a rationale which would integrate “nonsite” observations into anthropological problem solving. For example, I suggested (1974) that nonsites should be highly sensitive to questions of regional land use, particularly among hunter–gatherers. Championing element over cluster sampling, Thomas (1975) applied the same logic to his regional studies, whereas Dancy (1974) delimited land use areas in a New England context with nothing but “stray” finds and low

intensity scatters. Lastly, in a reanalysis of Willey's Viru Valley survey, Moseley and Mackey (1972) noted that problem solving was severely hampered by a focus on large sites to the exclusion of smaller sites and nonsites such as trails. The smaller sites should yield more activity-specific information and thus should be more useful for considerations of land use and behavioral interaction on the regional scale.

As more and more small and large survey contracts were completed under federal and state sponsorship, the question necessarily arose, To what extent would the information gathered by these projects contribute toward solving scientific questions that extended beyond the individual contracts? A solution to the atomistic nature of archaeological contracting emerged only in the Southwest, with the founding of the Southwestern Archaeological Research Group (SARG) (see Euler and Gumerman 1977, 1978; Gumerman 1971, 1973a,b; SARG 1974), and on a more limited scale in the Midwest with the inception of Struever's Foundation for Illinois Archaeology (Struever 1968, 1971, 1973). SARG incorporated among its founding members many regional centers of archaeological research. Laying aside their rivalry, these groups decided to pool their research results into a joint data base and to use future research to help address the question, Why did prehistoric populations locate their sites where they did?

While this question could easily be caricaturized as a least common denominator of low inherent interest, SARG was instrumental in spelling out an explicit set of variables upon which archaeologists should obtain observations, even though the cooperative effort was explicitly directed toward sites rather than distributions. The variables selected were bound to elicit a great deal of information regarding the causes of clustering in these distributions (e.g., F. Plog 1973; Plog and Hill 1971). In addition, SARG members were expected to record their data in ways that would allow comparison and hypothesis evaluation. Thus, it should help in specifying distributions more accurately in the future, in evaluating hypotheses about generating functions, and in permitting at least a certain degree of cumulation in research results. Archaeological performance elsewhere could have been significantly improved, if the SARG suggestions had been followed by others.

Compared to preceding periods, contributions to the methods discussed here expanded in scope. For example, chemical testing for the presence of buried occupation horizons via phosphate analysis was resurrected in a number of publications (Eidt 1973; Woods 1975), the chemical differentiation of cultural and natural features was outlined (van der Merwe and Stein 1972), and new methods for the identification of specific chemical distributions were introduced (Alford *et al.* 1971). Other prospecting methods continued to attract at least intermittent interest. Magnetometry was

championed as a means of detecting buried pits and even post molds (Gramly 1970), the differential payoffs from shovel testing and augering were investigated (Claassen and Spears 1975), a whole family of different prospecting methods was compared in an individual project (Trubowitz 1974), and aerial photography continued to be applied, though largely in areas with high surface visibility or as a way of stratifying a sampling universe more effectively (Gumerman and Neely 1972; Harp 1975; Matheny 1971; Vogt 1974). On a smaller scale, the efficiency of various field methods for observing the distribution of various specific artifacts and ecofacts was investigated in earnest, particularly among Higgs's students in England. The issue was not always decided in terms of the fanciest field method, but quite frequently in terms of which method would yield necessary and sufficient answers for the given question (e.g., Cherry 1975). Finally, the dilemma of those archaeologists was addressed who are faced with the excavation of a deeply stratified site, but nevertheless would like to obtain a representative sample of each of the components (Brown 1975). It was suggested that one should prospect (rather than sample) for those aspects of each component which belonged to the same systemic or archaeological context.

### 1976 to the Present

By the middle of the last decade, virtually all of the methods for the study of archaeological distributions had been developed already. However, this development had been spatially confined—it had been directed primarily to those few parts of the country where all or most of the archaeological record was unobscured by vegetation or so shallowly buried that periodic ground disturbance would produce a reasonably faithful image on the surface. This included the desert areas of the greater Southwest and those exceptional circumstances elsewhere, which combined plow cultivation with an archaeological record in the vicinity of the surface.

The past 6 years do not stand out at all because new methods were developed and new questions were raised. Instead, they are characterized by a process of digestion, translation, consumption, and modification of the extant repertoire of method in all those parts of the country where the conditions for specifying archaeological distributions were anything but optimal. This passive methodological stance was closely related to the fact that the vast majority of archaeological research was carried out within the framework of cultural resource management. Archaeological contracts did explicitly call for the specification of archaeological distributions, but they did not do very much to facilitate development and experimental validation of new method. Instead, they required the well-justified application of ex-

tant method and questions. The process of methodological fine-tuning in the suboptimal areas could be illustrated in any region outside of the desert Southwest. However, it is best exemplified in those parts of the country where the archaeological record is farthest removed from the eye of the observer on the present land surface, that is, in the northeastern woodlands.

Here the following situation pertains to archaeological distributions:

1. The region is heavily wooded. This implies that archaeological distributions are covered by a layer of leaf-litter and tend to have close to zero above-ground visibility even though they might be close to the surface. In Massachusetts, for example, this covers about 60% of the area surveyed during the last decade (Dincauze *et al.* 1981:Fig. 13).

2. Even a significant proportion of the agricultural land is not plowed but is in hayfields with similar barriers to surface visibility as in the first category.

3. Plowed land tends to be confined to the valley bottoms; here, even comparatively recent distributions tend to be buried under more recent alluvium.

It is not at all surprising that a large proportion of methodological research during this period was directed to the sampling of the subsurface. For example, in an article widely cited in those northeastern surveys which are explicit about their field strategy, Lovis (1976) suggested subdividing project areas on the basis of soils, relief, and stream dissection into three strata, of which a 20% sample should be intensively investigated with shovel test pits (one by one foot) every 100 yards along transects. In the test units, the vegetation mat and top soil were lifted out to the interface between humus and subsoil, and the adhering materials, the shovel content, and the interface were checked for cultural material. Clearly, with a sample fraction of 1/90,000 within a 20% sample, not much accuracy could be expected for the resulting estimates of site or artifact prevalence. Also, extant sites in the "sampled sample" would need to be larger than 140 yards in diameter to have been penetrated with certainty by at least one test unit.

A similar method was applied by Birk and George (1976) in a heavily wooded area in Minnesota. It was somewhat more intensive, however, since shovel tests were dug down to sterile soil, scraped with trowels, and pits were spaced every 5 m along transects. Plog (1977) suggested shovel pits at 20-m intervals along transects along the center of a highway right-of-way. Chartkoff (1978) similarly advocated shovel units at regular intervals, but radiating out from known find spots, to define the sizes of middens, sites, and artifact scatters. More idiosyncratic was his suggestion to employ the density per volume as site-defining criterion. Obviously, this value is not so

much dependent upon the density of cultural material but upon the prevalence of sterile matter around the artifacts. In other words, the suggested yardstick is of rather questionable utility.

The most complex subsurface sampling methodology was applied by Nance (1980). In an area divided into three strata, the strata in turn were subdivided into  $200 \times 200$  m grid sampling units, with the sample units in turn stratified into an east half, as well as northwest and southwest quarters. In each of the randomly selected grid units, the east half was surveyed by conventional walkover survey regardless of archaeological visibility, while 16 randomly placed  $1 \times 1$  m testpits were excavated to 10 cm depth in the southwest quarters, and 16 systematically placed  $1 \times 1$  m units (at a 25-m distance) were excavated to the same depth in the northwest quarters. While complex, the method controlled for archaeological visibility in regional sampling designs.

Aside from testpitting and shovel testing, corers (e.g., Luedtke 1978), and augers (Casjens *et al.* 1978; McManamon 1978) were recommended for exposing shallow archaeological distributions, and backhoes found their champions in exposing deeply buried archaeological remains (Chapman 1976). Phosphate testing also remained very much on the agenda when prospecting for buried sites and features (Eidt 1977; Sjöberg 1976; Woods 1977). Even the oft-neglected plow zone was resurrected as an archaeological resource by a number of investigators. They pointed out, for example, that (1) plow drag was not as severe a problem as had been previously reported (Roper 1976; Sterud *et al.* 1978; Talmage and Chesler 1977) so that it should preserve archaeological context to a limited degree; (2) however, plow zone collections would produce seasonally different results, depending on rainfall and agricultural disturbance (Hirth 1978); and (3) since the plow zone was so highly visible, its remains should be biased by their distance to roads and accessibility to collectors (Francis 1978; Lightfoot 1978).

Clearly, these methodological experiments did not concern themselves as much with sampling and estimation, but with improving the results of prospecting when sampling could not be avoided. Compared to these down-to-earth suggestions for prospecting, the advocacy of new and “fancy” prospecting method declined somewhat in the methodological literature. One article illustrated the productive use of magnetometer (see also Step-onaitis 1976), resistivity survey, and ground-penetrating radar in locating buried distributions nondestructively (Parrington 1979), and a few publications backed by sizable government funds advocated the use of aerial remote sensing, though largely for southwestern audiences (Lyons and Avery 1977; Lyons and Hitchcock 1977).

In hindsight, none of this specifically helped to overcome the problems

faced by archaeologists in the eastern woodlands. Instead, it added a few fine points to the plethora of methods which were employed already, and thus, it increased the methodological options for the imaginative archaeologist. The situation was not materially altered by a series of cookbook-like review articles on sampling and estimation. Their contribution to sampling methods was minimal (Ammerman *et al.* 1978; Burton 1977; Cherry 1978; Doelle 1977; S. Plog 1976; S. Plog *et al.* 1978; Rogge and Fuller 1977; Schiffer *et al.* 1978). What they did have to offer was a number of programmatic reminders about the sampling paradox, namely, that sampling and estimation can be intelligently applied only if we know something about the distributions which we sample, and that sampling takes off where archaeological knowledge leaves off.

Given the peculiar nature of the archaeological record in the north-eastern woodlands, what resources do we have that help us to anticipate the shape of archaeological distributions, whether we are interested in prospecting, sampling or estimation? This question was explicitly addressed by several northeastern investigators. For example, Dincauze (1978) pointed out that the variable of relevance was not the actual fieldwork, but rather the effort that went into the research design for this fieldwork. Because so little was known about archaeological distributions in the Northeast, it behooved the competent archaeologist to incorporate what little was known into the research design to maximum advantage. Without having to move a single ounce of dirt, one could approximate the expected shape of the given archaeological distribution by utilizing existing information sensitively—from bedrock to water supply maps, from aerial photographs to soil distributions, from ethnohistoric sources to local histories, and from actual “site” files to present or paleodistributions of fauna, flora, and other resources. Rather than a stab into the dark, the fieldwork would thus turn into an explicit attempt to evaluate one’s expectations and to modify one’s knowledge, in the face of fieldwork designed to falsify or increase this knowledge (cf. Barber and Casjens 1978).

An early application of this logic to archaeological problem solving can be found in Thorbahn and Paynter (1975). These authors derived an explicit model of prehistoric land use from rather similar lines of information as advocated by Dincauze. Their fieldwork then evaluated that model in their project area, with the result that both the land use model and the shape of the archaeological distributions over the project area could be specified with greater confidence. The research project combined the fine-tuned application of prospecting, sampling, and hypothesis testing methods to specific research conditions, with experimental work on generating functions. In this way, it contributed to knowledge about distributions which went well beyond the confines of their research area.

## THE FINER POINTS OF ARCHAEOLOGICAL PRACTICE

A review of the evolution of a specific body of theory and method would be incomplete without an analysis of its cumulative adoption and use in our profession. Such a review of practice should throw light on which of the “new” methods and rationales actually trickled down into day-to-day archaeological behavior, and to what extent their adoption affected the accuracy, reliability, and replicability of statements about archaeological distributions. An excellent point of departure for a look at cumulative practice is the Commonwealth of Massachusetts where a retrospective analysis of 220 archaeological survey contracts from 1970 to 1979 has just been completed (Dincauze *et al.* 1981).

One might assume that our image of archaeological practice would be distorted by focusing upon cultural resources management (CRM) activities only. However, for Massachusetts in the 1970s, there will be negligible bias for a number of reasons. For example, with a few noticeable exceptions such as Dincauze (1968) or Moorehead (1931), virtually no observations beyond individual sites had been obtained within the Commonwealth prior to 1970. Thus, our knowledge on archaeological distributions postdates the beginning of the last decade. Also, virtually all research on archaeological distributions in Massachusetts has been carried out under the umbrella of CRM. Thus, for all practical purposes, the population of archaeological survey contracts constitutes the universe of applications for this set of theory and method in the state. Similarly, the project archaeologists tend to be affiliated directly with the major regional centers of teaching and research in archaeology (64% of the reports reviewed are produced by individuals affiliated with the seven major academic centers of archaeology in the region). This indicates that what will be reviewed below is not the performance of a small band of archaeologists dedicated to CRM work only, but of the entire population of archaeologists concerned with the Commonwealth (80 different archaeologists were involved as principal authors of the reports). Finally, CRM archaeological reports are explicit contracts to provide information regarding the shape of archaeological distributions. Thus, the completed reports should be rather sensitive to the cumulative practice in those areas of theory and method under review here.

Regardless of the context of one's research on the shape of archaeological distributions, whether under contract or “pure,” one needs to carry out the following operations, ideally in the following order: (1) definition of one's project and project volume; (2) background work on archaeological distributions within project volumes; (3) preparation of research design; (4) fieldwork; and (5) presentation of results. *Project volume* as the term is used

here is that arbitrary unit that is bounded horizontally by the given contract specifications and bounded vertically, at the top, by the present land or structural surface and, at the bottom, by either the depth of penetration of adverse project impacts or by the maximum potential depth of archaeological resources. My analysis of cumulative archaeological practice parallels this sequence of operations.

### Definition of Project Volumes<sup>1</sup>

Although processually rather vacuous, the reader nevertheless needs to know for what area the investigators considered themselves responsible (an average of 59%, from 36% pre-1976 to 100% by end of review period), and in what parts of this area a given project will impact the project volume to what horizontal (35%) and vertical (27%) depth and to what intensity (less than 20%). Even where this information might have been explicit in a project's scope of work, it should have been repeated in the completed report so that the reader could have evaluated the necessity and sufficiency of the steps taken to define the shapes of the given archaeological distributions.

### Background Work

When concluding background research, it is certainly desirable to be able to anticipate the shape of the relevant archaeological distributions in as much detail as possible. We examined the extent to which known information about the project area was actually incorporated into the reports (presented here as 10-year averages). This included information about preexisting archaeological resources from site files (used in 86% of the reports), land owners (53%), historical societies and commissions (52%), museums and collections (40%), avocational archaeologists (39%), and professional consultants, be they archaeologists (17%) or others (6%). Also of interest are sources of information that relate not so much to what is known about the archaeology of a given area, but to what is knowable given a search by the investigator. For example, the ethnohistory (32%) or history (91%), the socioeconomic development (46%), the history of land use and disturbance (60%), and the standing architecture (48%) may predict what once was there but is no longer visible on the surface. Similarly, the present environment in terms of water sources (68%), topography and geology (59%),

<sup>1</sup>Figures in parentheses indicate what percentage of the reports actually provides the stated information.

vegetation cover (48%) and soils (57%), or past environments (26%) may help in anticipating past archaeological distributions. Finally, one would like to know if anything could be learned, if only in the broadest outline, from general sources in history (82%), prehistory (66%), methodology (14%), or management (53%), or from archaeological surveys in areas close by (29%).

Our scale of evaluation of background work was only modestly demanding of investigators' performance. Nevertheless, the mean overall performance in this operation amounted to less than one-half of the possible score. Even a figure of 100% would have been potentially misleading, however, because the mere mechanical accumulation of preexisting data of relevance is not the scientific part of background research. At best, it is one of many tasks to be accomplished before scientific work can begin. The challenge of background research lies elsewhere. On the one hand, it consists of an evaluation of whether what is known about the project volume is reliable, accurate, and robust. On the other hand, it is the extrapolation from what is known to what is knowable, the definition of what is presently not known but needs to be known, and the specification of what these kinds of considerations mean for the distributions of interest.

Scientifically speaking, background research is irrelevant if it is mistaken for a mechanical task, a token interlude between the signing of a contract and the beginning of fieldwork. The behavior of the profession on the average was noticeable for the relative lack of depth and breadth in background research. What should be a mere starting point toward characterizing distributions beyond the few points where they have been or can be observed, usually becomes an end product. Clearly, if background research is to be of maximum benefit, it needs to be more than boilerplate. It requires its own problem-focused research design carefully in tune with the specific research goals.

## Research Design for Field Tests

To accomplish anything useful with archaeological distributions and their shapes, a design for field tests should (1) evaluate whether what is known about distributions is accurate, (2) increase accuracy where necessary, (3) formally test what is only anticipated, (4) resolve competing expectations, and (5) acquire firsthand knowledge where none exists and where nothing can be specifically anticipated. In addition, research designs need to justify and spell out in sufficient detail the methods needed to attain these goals. This should allow readers to replicate it in their minds and to inde-

pendently validate its logic. We looked at approximately 20 different variables under this heading to see what they might have contributed toward these ends.

With background research insufficiently problem directed, the archaeologists in our population were usually forced to treat their research area virtually as *terra incognita*. Nevertheless, this did not usually prevent them from subdividing their contract areas into different strata. For example, an average of 20.5% of the project areas was excluded from any field testing because they were considered inaccessible or disturbed beyond repair. Since every New England distribution is disturbed in one way or the other and since, in most cases, the disturbance tends to be more apparent than real as, for example, with plow disturbance, it should serve as an unevaluated basis for exclusion only where the entire vertical and horizontal dimension of a carefully defined area is known to have been replaced or removed.

Archaeologists also rather liberally excluded some parts of their research areas from further consideration which, in their estimation, had close to zero sensitivity for archaeological remains of any kind. For example, about one-half of the project area considered accessible and undisturbed was so excluded in the 94 reports that combined background research with field tests. Moreover, in about two out of every five cases, this was done entirely without justification. In our present state of knowledge, such exclusions are dangerous. They will tend to amplify whatever biases exist and systematically write off those parts of the archaeological record which, for historical reasons, are either seriously underrepresented or not included at all in our extant observations.

About 3 out of every 10 archaeologists did their background research thoroughly enough to be able to stratify their project areas by sensitivity, that is, by the expected prevalence of archaeological remains. Yet, even this low score is misleading, given the peculiarities of the archaeological record in Massachusetts. For example, site densities observed by archaeological fieldwork in the state do not vary with the specific variable which is most frequently used as the basis for stratification, environmental zone (60% of the time; in land—upland, inland—riverine, etc.). Instead, site numbers vary primarily with the size of the sampling fraction (positive correlation) and the size of the area surveyed (negative correlation because sampling fraction and size of survey area, in turn, are negatively correlated). Thus, the known archaeological record is a relatively poor guide for stratification procedures. In our present state of ignorance about what affects distributional shapes, this standard criterion for stratification does little more than operationalize our ignorance. Even where expected sensitivity can be explicitly derived, the given stratification logic needs to be evaluated by field test. Only in this way

can we guard ourselves against the possibility that other criteria for stratification, including entirely random ones, would track the archaeological record more elegantly.

There are very few examples in our population where stratification leads to, or is derived from, expectations that postulate anything beyond “more” or “fewer” archaeological remains than elsewhere. This is obviously insufficient, since it does not help us to select an appropriate prospecting or sampling methodology. Only about 20% of the investigators who stratify their research areas or who predict anything about their project volumes provide *any* of the following information: (1) range estimates of the expected average densities; (2) expected degrees of aggregation and dispersal; (3) orders of magnitude of spatial correlation or association of the expected resources with each other or with specific characteristics of their area; (4) rough ideas about the size, shape and visibility of the resources; and (5) some definitions of the size shape and internal clustering of clusters of resources (“sites”, “features”). Without this information, the choice of sampling or prospecting strategies is little more than a shot in the dark.

By some unstated convention, 65% of the archaeologists in our population used a systematic aligned sampling strategy on a square or lineal grid (with another 5% preferring rectangular or triangular grids). This choice is relatively auspicious, of course, since, minimally, it permits statements such as “Given that we do not know anything about the shape of the distributions that we sample or in which we prospect, and assuming that all clusters will become visible if they are intercepted by our sampling units, we can at least state confidently, that we will not miss clusters larger in diameter than the maximum distance between adjacent grid units.” Also, such sampling units tend to provide a systematic reading of spatial variation. In other words, although rarely justified in these terms, the mechanical adoption of this one sampling design by the vast majority of investigators assures that at least some statements about the distributions can be replicated.

In contrast, only 7% of the field tests employed simple or stratified random designs. On the one hand, if we know next to nothing about the distributions we sample, we do not know how to interpret what we observe in terms of distributional shape. On the other hand, we lose our ability to derive the kinds of bounding statements that are possible with a systematic aligned design. Nevertheless, simple randomization forces us to observe distributions in an explicitly unbiased way. This is obviously superior to intuitive (11%) or altogether unspecified (3%) sampling designs. Transect sampling was employed in only 8% of the cases. It is located somewhere between the extremes of systematization and alignment versus randomization. A transect tracks spatial variation like a systematic grid, but, like

random designs, loses out when it comes to the development of bounding statements.

In these designs for subsurface testing or prospecting, 22% of all field test strategies were applied with a target or goal specified in advance. Slightly more than one-half of these, again, were accompanied by at least some discussion of the general likelihood of detecting particular kinds of resources. Of course, this likelihood is very much dependent, not only on the type of resource expected and on its size and visibility characteristics, but also on sampling fractions, sample sizes, and test unit sizes and shapes. Where sampling fractions were or could be specified, about 80% are between 0.001 and 0.00001 (5% are more than 0.01, 4% are between 0.001 and 0.01, 44% are between 0.0001 and 0.001, 35% are between 0.00001 and 0.0001, 10% are less than 0.00001). By southwestern standards where these methods were first introduced in archaeology, these fractions are pathologically small. Additionally, the predominant field test units are extremely tiny; 42% of the time, a shovel test pit measured  $50 \times 50$  cm on the sides (then, in order of frequency: 23% of the time,  $75 \times 75$  cm; 16% of the time,  $25 \times 25$  cm; 12% of the time,  $1 \text{ m}^2$ ; 7% of the time, greater than  $1 \text{ m}^2$ ). One-third of the time, the size of test units was not specified at all. From these two figures, it is easy to derive an average test unit interval of 24 m for the modal sampling design of systematically aligned square grids. Shovel tests were employed in 68% of the contract areas, the remainder largely taken up by various coring strategies to detect culturally produced subsurface anomalies (22%). Interestingly, the average coring interval is only 11 m. Mean sample size was 88 test units.

In the research designs scrutinized here, there tends to be a negative correlation between the size of strata or survey areas and the intensity of projected field methods and tests, quite independent of any stated problem orientation. In large strata or project areas, the interval between test units is three times larger on the average, and shovel test sizes tend to be 50% smaller than in small strata or survey areas (Dincauze *et al.* 1981: 145–147). At least in part, this must relate to a presumed negative correlation between the size of project areas and financial support per unit area of projects. At the same time, where one expects to find nothing, sample intensity and sampling fractions are reduced. This can very easily become a self-fulfilling prophecy which would amplify preexisting biases in the archaeological record.

Given the predominant lack of justification for the methodological choices that logically precede it (e.g., stratification, sampling design, sampling fraction, sample size, test unit size and test unit spacing), it should come as no surprise that the vast majority of research designs does not provide

justification for the method used to process excavated matrix. No information was provided on what was done nor on what was supposed to be done with excavated matrix in 24% of the reports. Another 23% of the field tests explicitly do not use screens. By some magical if unstated convention, if screens were used, they were  $\frac{1}{4}$  inch (0.6 cm) mesh 84% of the time. Diverse and rather marginal techniques were used to supplement the information from the test units, and they did not constitute trend-setting innovations.

Several points emerge rather starkly from this review of research designs during the past 10 years in our professional population. In their mode, if not their range, (1) they were uninformed by background research that could have helped to anticipate the (shape of) distributions adequately; (2) even where adequately anticipated, research designs failed to demonstrate the adequacy of stratification criteria; and (3) strategies remained largely unjustified boilerplate for sampling design, sample fraction and size, test unit size and spacing, and excavation and postexcavation methods. The suspicion arises that the modal field strategies may amplify rather than reduce existing biases about archaeological distributions in the state.

## Fieldwork

Once the research design is fully specified, the actual fieldwork is *almost* fully determined. However, fieldwork does need to go beyond what is specified in the research design in at least three different directions. First and somewhat paradoxically, unanticipated field observations must be anticipated. Poor archaeological visibility combines with small observational windows to force us to supplement “nonzero” observations in the units of our research design with observational detail in their surroundings, if only to be able to evaluate them. Second, tests should be executed at least as carefully as mandated by the research design, adding an aspect of quality control to field work that none of the reports address explicitly. Where execution in fieldwork varies in an uncontrolled fashion and falls below the bottom line required in the research design, the observations do not track the distributions of interest, but float. Only determined quality control will assure that observations surpass what is sufficient and necessary in accuracy, consistency, and replicability.

Finally, CRM fieldwork has an adverse impact on archaeological resources. Thus, beyond what is necessitated by their personal research designs, investigators carry the heavy responsibility of minimizing the destruction of “nonrenewable” observations, a point which is addressed in few of the reports. This means that sufficient provenience and contextual information need to be provided about each distributional detail, whether or not it

is part of research design. This is a minimum requirement for the reconstruction of each detail's locus and associations. In this way, future archaeologists can partially overcome the adverse impact, whether the impact is the result of the research contract or of the project whose impact the contract is intended to minimize.

### Presentation of Results

If the necessary observations have been fully specified and justified in the research design, and if these observations were actually carried out and, if necessary, supplemented by the kinds of information suggested previously, then the presentation of fieldwork results might be considered rather mechanical, scientifically speaking. Minimally, all field test units need to be located on maps of appropriate scale; their shape, size, and orientation need to be defined; and the depth of penetration and the intensity of treatment of the deposits need to be spelled out. Then, we need to know what information was recorded in the field and developed in the laboratory. Finally, reporting should be expanded from broad averages across major strata or entire project areas to reporting of all relevant observations by provenience and in their distributions.

Most investigators in our population consider their work done as soon as they have reported all of their observations. In this way, what should be a mere stepping stone toward increasing our scientific control over archaeological distributions is mistaken to be the desired goal and end product of scientific research. The true scientific challenge begins only where the recitation of observations leaves off. The archaeologist needs to address some of the following questions. What do these observations mean in terms of the *distributions* anticipated by background research and research designs? What has been learned that was not known before? Where do we have to revise our anticipations? And where can we specify distributions more accurately than before? All these questions are instances of the broader question: What have we learned from our observations about the *entire project* volume, rather than merely about that small fraction of it which we have actually observed?

As soon as the logical force of the anticipations has been developed by background research, translated into the research design and evaluated, revised and supplemented by fieldwork, that force needs to be called into service for estimation. Compared to a carefully justified estimate of distributional shape for the entire project area or stratum, the unbridled presentation of fieldwork results is little more than vacuous description. The set of "zero" and "nonzero" observations needs to be assessed against the uni-

verse of all possible sets of observations which theoretically could be obtained from the given distribution. Without that assessment, all of the following questions remain unanswered.

1. Given what we know about that distribution, how faithfully does our specific set of observations mirror its shape?
2. If we changed our assumptions, how would that change the precision with which our observations specify the shape of that distribution?
3. Conversely, if we don't know what kind of distribution is actually sampled, of what kinds of distributions could our observations possibly be a part? What kinds of distributions would be totally out of the question?

The kinds of estimation methods that would help to answer some of these questions were used only in a token number of reports. Where the reports do provide estimates from the observations about what has not yet been observed, they merely clone the extant observations across the unobserved space. Boilerplate estimation merely determines the mean number of observations per observed unit space and multiplies it by the total area under analysis. Statistically speaking, such estimates do *not* provide the best estimate of prevalence for the distribution in question. More disturbingly, the modal procedure further amplifies the traditional bias underlying the observed archaeological record, that is, a disproportionate emphasis on clusters (justified by "zero" observations elsewhere) at the expense of the continuity of archaeological distributions.

Where methods are insensitive to the detection of low densities, low densities will be observed as zero, while only very high densities will predictably produce nonzero observations. If these same observations thereafter are multiplied by a constant, zero multiplied by a positive constant will remain zero, while a nonzero constant multiplied in the same way will increase in size. Based as it is upon arithmetic means that are particularly sensitive to outliers (zero counts and very high counts), the standard method of estimation cannot but exaggerate our inherited notion that archaeological distributions are highly clustered, with clusters separated from each other by vast behavioral and observational vacua. Moreover, since extant observations unfiltered by acceptable estimation procedures add to the predictive store with which we can fine tune our anticipations, observations unfiltered by acceptable estimation procedures will introduce uncontrollable noise into the background research and research designs of generations of future archaeologists. Given the distribution from which one's sample comes, one's sampling design, and one's chosen level of precision, the specif-

ic estimates are not any more likely to be accurate than any others within the range of estimates derivable from sampling theory.

## THE SHAPE OF ARCHAEOLOGICAL DISTRIBUTIONS IN MASSACHUSETTS

### Notes on the Ethnohistory and Ethnology of Archaeological Method

The ethnography of archaeology, as it applies to the use of method and theory in practice, remains yet to be written. The preceding pages may serve as a stimulus for others to look at other problems, other periods, and other areas. The driving forces underlying the observed behavior of archaeologists do not necessarily relate to internal ferment and contradictions within the discipline itself. Rather, they are largely external to it, namely:

1. The external environment of the discipline, in the form of federal and state legislation, provided a new niche for the funding of archaeological activity such as CRM specifically calling for the specification of archaeological distributions.
2. Archaeologists responded by adapting their research questions to this new niche (the study of archaeological distributions in their home state).

At the time the new niche was created, the extant archaeological theory and method had not been sufficiently developed to provide the most elegant answers to the new questions that were now asked of the discipline. By 1970, the beginning of the decade under review here, virtually the entire state-of-the-art methodology applicable to the new niche had already entered the nationally visible literature in one form or the other. At the same time, the new research context, at least in Massachusetts, while freeing considerable funds for applied archaeological research, also imposed new constraints. These constraints included the following:

1. Very short deadlines followed the announcement of a given project, the signing of the contract, and the completion dates of background work, field research, and the presentation of the results.
2. Research areas were determined by outside forces rather than archaeological expertise.
3. Financial resources, though sizeable, certainly were not adjusted to take into account the peculiar nature of the northeastern woodlands.

Faced with these constraints, archaeologists did not distinguish themselves by developing new method optimally suited to the new problems. Instead, they searched the extant set for what it might have to offer, particularly since the whole problem setting was of the “applied” category. Those who reviewed the performance of these investigators could not encourage experimentation at the cutting edge of the state of the art. Rather, they needed to insist on method that had been maturely established (and thus would not jeopardize deadlines or answers). They also had to enforce certain minimal standards of performance—standardization of any kind being the archenemy of method fine tuned to specific problems. The cumulative product generated under these circumstances showed a choice of rationale and method that satisfied what was considered necessary and sufficient by the sponsors, though the choice was not optimal, much less maximal, that is, the context selected for consumption of what was established, without significant innovation. One suspects, the same behavior may well characterize cumulative performance in any category of research that is strongly dependent on outside funding.

In the problem setting under review, this translated into the wholesale adoption of a *rationale* in which the archaeological record is viewed as discontinuous, with clusters (“sites”) separated by vast stretches of archaeologically empty areas. This was the inherited paradigm of preexisting theory and method. In terms of archaeological practice, it would be difficult not to find at least some clusters, regardless of methodological elegance, if they are there in any number and if they are sufficiently large, dense, and visible. In terms of the sponsors, they will receive a product, sites, which can be controlled and managed as long as it is viewed as finite, with definite bounds. Moreover, sites (i.e., the peaks of artifacts and features) make it easier to satisfy “significance” than any other parts of the distributional continuum: where lots of (different) resources are concentrated, at least a few should relate to the research questions of the day. Lastly, “sites,” as an archaeological phenomenon, harmonize smoothly with the paradigm of the sponsors, the state and federal government and the public.

At least as important to this internally consistent rationale is the difficulty with which the investigators or sponsors can be proven wrong when they view archaeological distributions as a dichotomy between sites and nothing. Other distributional parts, equally as interesting, are not likely to be recognized during the impact period that follows the field investigation, or by the avocational archaeologists to whom the area ultimately returns after the completion of the given contract.

The set of *methods* chosen should be consistent with the constraints upon the investigator, with “normal” science, and with the rationale. For the investigator, sampling promised quick, mechanical, quantitative, and,

thus, exact answers. By the end of the 1960s, sampling had become normal science and it is little wonder that it became the panacea of the 1970s. For the sponsor, the numerical answers generated by sampling made it possible to administer the archaeological record: they can be compared, contrasted and accumulated on a single scale, which allows management decisions to be objectified if not mechanized. Moreover, if the scale is numerical, the sponsoring agency can more easily quantify its own numerical product and illustrate its annual activities with hefty increases on a cumulative curve. This sits well with legislatures and with the public that supports both legislators and sponsoring agencies. Thus, the almost universal adoption of sampling appears entirely consistent with the matrix in which it was adopted.

Yet, there are important internal contradictions hidden behind this image of smooth integration. Sampling methods entered archaeology by way of the desert areas of the Southwest. Where one can assume that virtually the entire surviving archaeological record is accessible on the surface, unobscured by vegetation, it is only natural that sampling would have been preceded by a lengthy period in which the *entire* archaeological record then of interest would have been recorded and reported at least in some areas. By the time the survey tracts became so large that it had become impossible to survey them in their entirety, a shift to sampling was unavoidable. What is of significance here is that

1. Many “complete” subregional distributions of sites had been mapped already that could serve as models for stratifying the survey areas.
2. The distribution of site types and site sizes was relatively well known for the same reasons; thus, the size of sampling units could be chosen to optimize the discovery, observation, and definition of sites.
3. Survey conditions were so optimal that large sampling fractions, large sample units, and/or large numbers of observations were the rule rather than the exception. As a result, even without the application of statistical estimation procedures, the resulting observations were therefore relatively sensitive measures of distribution, if only of sites. Also, with an archaeological record so easily accessible, the results were far easier to verify independently.

To transpose this methodology in its entirety into the Northeast and to apply what had been developed for surface collecting to subsurface testing introduces severe problems—problems, moreover, that are not easily overcome within the regional paradigm. One problem was that no “complete” subregional distributions of sites were known in the area before the beginnings of the last decade, nor has the frantic activity of the last ten years produced any. This means that the distribution of sites needs to be consid-

ered “unknown”; conversely, the research designs cannot be fine tuned in terms of stratification criteria which are optimally sensitive to site distributions. Also, the distribution of site types and sizes for the Northeast is not known. Hence, there is nothing to guide us in choosing an optimal size or spacing for our test units. Finally, subsurface survey is so time- labor- and money-intensive that sampling fractions, the size of test units, and the number of samples must be smaller by several orders of magnitude.

### Sampling as Prospecting

With a rationale that views the archaeological record as clusters separated by sterile space, and with sites as the aspect of distributions that needs to be specified and managed, it is not surprising that sampling as a method should find its principal use as a prospecting tool to locate sites, rather than as a method to collect appropriate measurements so that distributions could be specified in a controlled way. How good is sampling for this task given the modal sampling design in Massachusetts? What is the chance that the modal sampling design will intercept “sites” with anything approaching certainty?

Let us assume that “sites” can be recognized by a single hit, that sites are circular in shape, and that sites are not surrounded by space which would be mistaken for sites in shovel tests. Given our modal sampling design, with a  $50 \times 50$  cm test unit every 24 m on average, our subsurface surveys would intercept only features larger than  $900 \text{ m}^2$  (diameter 33 m) with certainty. A common rationale behind the given shovel test interval is to intercept phenomena of the same size as the area enclosed by four grid points. The modal design will miss these more than 29% of the time. If we were interested in locating smaller clusters such as Algonquian wigwams and their archaeological remains (3-m radius), our chance of locating them decreases to less than 5%. A fireplace, 1.5 m in diameter, would be missed in 99 out of 100 cases. Clearly, even if every test unit that intercepted a feature would provide unmistakable evidence that a feature had been intercepted, our prospecting success would be rather low.

This brings up another interesting contrast between the producer and consumer area of this set of methods, which might severely hamper the ability of the modal sampling method to find sites. In the Southwest many areas have had long-standing sedentism with its associated features and facilities. Sedentism is bound to cluster behavioral and archaeological distributions significantly more strongly than the more ephemeral settlement patterns of northeastern populations. Moreover those settlement patterns are not accompanied by equally persistent structures and facilities. This

contrast is accompanied by the previously mentioned very real difference in the size of test units— $50 \times 50$  cm versus  $500 \times 500$  m—that is, by six orders of magnitude!

There are virtually no archaeologically graspable behaviors that maximize their spatial variance when they are observed in units as small as those customarily used by northeastern archaeologists. It is quite possible, then, that what seems to work well in the Southwest, that is, the discovery of clusters by means of sample surveys, might not work so well if it is transposed into the Northeast. Where sedentism creates large and dense clusters, with structures and facilities, a large sample unit could not fail to expose sites. In our sampling designs, the explicit assumption is that our modal  $0.25\text{-m}^2$  pit, if placed into a site, would produce a nonzero artifact count or other evidence of cultural disturbance, and that the grid is dense enough so that a site would be hit by at least one grid unit. Granting the latter assumption (although, as the previous paragraph indicates, that assumption is usually violated), let us evaluate the former.

Let us assume that a given site is penetrated by at least one test unit, and that artifact counts are distributed randomly across that site. Under these assumptions, a site would need to have a mean artifact density of more than 12 artifacts per  $\text{m}^2$  (Poisson distribution) to obtain a count of 1 or more, in 95% of the cases. Assuming that an archaeologist would interpret the find of a single artifact as an insignificant stray, the site would need to have artifact densities greater than 19 per  $\text{m}^2$  to yield observations higher than 2, 95% of the time. If we want to be only 50% sure to get a nonzero (non-zero and non-one) count from our unit, its mean density still would need to be greater than 3 per  $\text{m}^2$  (7 per  $\text{m}^2$ ). Contrast this with typical artifact densities within well-surveyed sites in the Southwest, where the average count per  $\text{m}^2$  is frequently around 3 per  $\text{m}^2$  in populations of sites, and where, on occasion, 95% of all sites within a surveyed area have densities lower than 5 per  $\text{m}^2$  (S. Plog *et al.* 1978).

The previous examples assume that all artifacts in the matrix actually would have been encountered and that all potentially artifact-bearing strata have been penetrated by the test unit. Assuming, optimistically, that without the use of screens 75% of the artifacts would not be recovered and that only 50% of the potentially artifact-bearing matrix would have been penetrated, we would not be wrong more than once in 20 cases if we interpreted such observation as deriving from distributions between 0 and 72 artifacts per  $\text{m}^2$ ! Conversely, let us assume that sod turns are the method used in subsurface testing ( $25 \times 25$  cm, 17% of the time), that deposits are screened, that artifacts are randomly distributed across a site, and that the artifact density of the site is somewhere between one and two counts per  $\text{m}^2$ . There would be a chance of about 5% that 32 consecutive sod turns would yield zero

observations. If deposits were not screened and the full matrix were not penetrated (as is common with sod turns), so that, conservatively, 90% of the artifacts escape detection, the chance becomes better than one-half that 64 consecutive sod turns will observe nothing. The chance that a given sod turn actually produces a count greater than zero decreases virtually to zero.

Corers and augers are not much help either. In Massachusetts, with its ephemeral regional settlement patterns, the proportion of site areas taken up by recognizably "cultural" soil disturbances is unknown but is bound to be low. Where these devices produce only undisturbed ground, it may tell us little about the prevalence of sites, given the small size of our window, the wide grid spacing (more than 11 m on average) and the fact that they rarely penetrate all potentially sensitive deposits.

One might place one's hope on the expectation that numerically high observations, at least, would have some predictive value for indicating that they would derive from site clusters, that is, that they would be surrounded by test units that, if excavated, would generate counts as high as or higher than those observed in our grid unit. But there is nothing in the method itself that allows us to predict from a single measurement, be it high or low, the nature of *specific* other measurements. In fact, after obtaining a set of observations from a given area (not subdivided into further strata), sampling *per se* allows us only to make predictions that are exactly the same for any other potential observation. In other words, spatial sampling allows us to make statements about distributions of counts, measurements, or attributes that have not yet been measured, but without reference to their specific locations! Conversely, a given sample reading, positive though it might be, is of predictive value solely for the universe of possible observations that could be obtained in a given survey, not as a guide to specific other high readings (as is implied if sampling logic is employed to identify and locate sites).

This discussion points up a massive contradiction between the extant rationale (the expectation of a dichotomous archaeological record consisting of sites and sterile ground around them) and the modal method that is used to generate observations on that archaeological record. Given that a specific site is not likely to be hit more than once by a test unit, there is nothing in a single numerical observation that allows us to differentiate false negative observations from true sterility. Since the chance to obtain zero observations from distributions of considerable artifact densities is rather high, the surroundings of such a unit would have to be searched in the same way that one searches the surroundings of units in which a high count was observed. Similarly, the chance that the surroundings of a given high count would be close to sterile also cannot be rejected offhand, given the relatively large spread around observations of counts that derive from distributions

with a low mean. That is, false positive observations are as difficult to recognize prior to investigating their surroundings as false negative readings, and the surroundings of *any* observation would need to be searched in order to determine whether or not *sites* have been encountered. As a matter of fact, it is quite probable that the distribution of counts generated by the modal sampling design, given wide grid spacing and behaviorally irrelevant small test-unit size, approaches a Poisson distribution even where the grid covers some obvious sites with the areas between them. In other words, sampling used in the service of prospecting does generate numerical answers, but, given the peculiar conditions of Massachusetts or the eastern woodlands, we simply do not know which questions are answered by the numbers.

### Sampling and Estimation

Ironically, whereas sampling is not particularly useful in finding sites as the paradigm would have it, it should be of help in estimating the shape of archaeological distributions. The shapes that the method should help to generate are only spatial ones in the sense that a given distributional estimate refers to a given spatial stratum, not in the sense that it mirrors the distribution of counts from unit to unit across the stratum. The prerequisites for effective use of sampling are (1) definition of strata, so as to minimize the variance of counts within them and maximize the variance between them; (2) sample fractions larger than a baseline minimum; (3) reasonably large sample sizes; and (4) a sampling design sensitive to the phenomenon it observes. With these prerequisites fulfilled, sampling does allow us to estimate the shape of the distribution of unit counts from the stratum, and the range of possible population (universe) values from which the given sample could derive if chance were the only variable that determined the presence of our sampled units in the sample. One needs to remember, however, that these estimates are controlled ones only for the given unit size at which the stratum is sampled (e.g., they are poor estimators for sites if the test units are tiny relative to sites).

If small sample sizes (i.e., low numbers of sampling units) are combined with large strata, small unit size, and/or small sampling fractions (characteristic of New England), one cannot expect to arrive at estimates that are sufficiently accurate and precise to be useful. This is particularly so where the phenomenon about which we want to make statements is extremely rare. For example, in Massachusetts, on the average test units with counts greater than zero do not occur more often than 1 in 120 for all strata combined. Were we to assume, for simplicity's sake, that the proportion of

nonzero counts in this universe is, by order of magnitude, similar to that observed in our sample, and that we were interested in estimating this variable, we would need a sample size of more than 10,000 observations to achieve such an estimate with a coefficient of variation smaller than 10% (Cochran 1977:54–55). In other words, we would need more than the actually available field observations from all of Massachusetts over the last decade, to come up with reasonably precise estimates of the prevalence of “site” counts in the universe. The average sample size in combined background and field-test surveys is a mere 122 test units.

It is clear that the variance in our estimates due to small sample size overwhelms any variation attributable to behavioral and other variables! Thus, we do not gain by comparing the observations obtained from different strata. Conversely, the cumulative field-test results obtained so far cannot be very precise measures of site prevalence, nor of distributions of archaeological resources across the state and across strata, if significantly more observations are needed to provide such estimates within reasonable limits of precision, with reasonable confidence. Since, moreover, we must assume that the individual areas sampled differ significantly from each other, the confidence intervals around our estimates increase in ways which simply cannot be controlled statistically.

With sites singled out as the “significant” aspects of the boilerplate rationale, it is not surprising that systematic sampling on a square grid has become the boilerplate method. It would, indeed, serve as a good method for estimating site prevalence if (1) sites were so large that they would be intercepted with certainty by at least one test unit, (2) the distribution of observations within sites, at the sample unit size, would overlap the distribution of observations outside of sites only to an insignificant degree, and (3) for a given level of accuracy, the costs in time, money, and personnel would be lower than with other methods. We have already demonstrated that assumptions (1) and (2) are violated in an uncontrolled fashion at this time. However, even if (1) is violated and (2) clearly does not hold for a significant proportion of the kinds of sites that ethnohistory tells us should be found in New England’s archaeological record, systematic sampling nevertheless allows us to make the following kinds of statements about the population from which we “sample.” Given the trigonometric characteristics of our sampled units, we can state with confidence, nay certainty, that we will not have missed sites whose inscribed circle is larger than the circumscribed circle around adjacent observation units, as long as “site” presence can be inferred from the observation generated by a single hit of the site by a sample unit, and as long as the sampling units penetrate the entire matrix of potential relevance to our question. Even where grid units are spaced signifi-

cantly farther apart than would be necessary to intercept a given size of site with certainty, we could still derive the following kind of estimate. Given the same assumptions mentioned previously and assuming that sites are randomly distributed relative to the sampling grid, we could estimate the chances of having missed a site of given size. This is not much, but it would be helpful to have field-test results routinely evaluated against this filter.

One could improve upon the utility of systematic sampling in a minor way, by replacing square grids with equilaterally triangular ones. They are as easily laid out in the field, yet for the same number of test units per given space, they intercept significantly smaller circles with certainty. Since the maximum distance from sampling points is smaller in equilateral triangles than in any other point configuration, they also tend to track spatial distributions more sensitively than square grids. Finally, in a triangular area, with equilaterally triangular grid unit, more area is sampled per point, on the average, than if the grid points of a square grid are arranged over a square area (cf. Dincauze *et al.* 1981:185). Yet, this is a small consolation, compared to the virtual impossibility of evaluating the given observations for site presence, whether or not they are positive or negative.

The conclusion is difficult to avoid that a decade of sampling in the service of specifying archaeological distributions (i.e., site distributions) has not been very helpful:

1. Given ignorance about the shape of archaeological distributions in the Commonwealth, as well as small sample sizes, small test units, and small sample fractions, the method per se is of no particular relevance to the reason for its widespread adoption, that is, to the goal of locating and defining sites.

2. Given the low proportion of test units that yield anything but negative observations as well as small sample sizes, the method is much too imprecise for defining the shape of archaeological distributions, much less for estimating, by order of magnitude and in a replicable manner, the range of imprecision around our estimates.

3. To the extent that the results obtained by sampling have been used as input to fine tune later research designs, observational noise has been amplified instead of controlled. Ironically, this process has amplified exactly those sources of bias, from which the preexisting archaeological record had suffered, that is, the preoccupation with the densest, most highly visible, and most clustered aspects of the archaeological continuum.

Clearly, all of these conclusions are based on rather conservative assumptions. The reality of archaeological practice would tend to further decrease the confidence with which we can utilize the set of extant observations.

## The Contents of the Screen

What has been learned from this universe of archaeological practice about the shape of archaeological distributions in Massachusetts? Our review of over 200 reports on distributional shape suggests two answers: (1) our corpus of CRM contracts constitutes the predominant data source on archaeological distributions in the Commonwealth, and it has significantly increased our knowledge of the variability in space and time of archaeological resources; but nevertheless, (2) our ability to anticipate the shape of these distributions, or even our knowledge of the shape of already-observed distributions, has not increased in any major way.

Little if any work on distributions (beyond the confines of the individual site) had been done before the decade under review; and even during that decade, virtually everything that was done was contained between the covers of the reviewed reports. Thus, quite obviously, the CRM reports have significantly increased our knowledge about distributions. This is most visible in what we know about the size and variation of the archaeological record. For example, the number of known archaeological sites almost doubled in the areas where contract work was performed.

Since contract areas are distributed across the state in terms of criteria relatively independent of preexisting archaeological biases, observations should have been obtained in a number of environmental zones at the macro- or microlevel, which were not previously known to generate them. As contracted, much of this work employed far more systematic techniques of areal coverage than had been standard before. This should result in a much more concise definition of specific resources by shape, size, location, and internal, contextual information. It should also uncover resources of low density or visibility somewhat more sensitively than avocational techniques.

As a rule, contract reports receive only miniscule distribution and are very difficult to access. Nevertheless, they must be rapidly completed and released. This had the effect of increasing the publicly accessible parts of the archaeological record by several orders of magnitude (and probably gave Massachusetts one of the most favorable ratios between what has been observed and what is in print about archaeological distributions). Certainly, a number of new methodologies and approaches were applied for the first time in the area, again increasing the resolution and producing a better sense of what could yet be uncovered.

While our store of observations on what is there has been vastly increased, our ability to predict what might be there but has not yet been observed has not necessarily improved. On the basis of our extant observa-

tions, we simply cannot say where archaeological resources are more likely to occur than elsewhere, what shape the distribution of any given resource might have where it has not yet been observed, and what kinds of variables affect distributional shapes. Sites discovered by contracts simply do not pattern themselves ecologically, but solely as a function of fieldwork intensity and test stratum size. Without adjusting for sampling intensity (and without controlling our estimates by statistical methods), we simply cannot translate observed high densities in a given area into anticipations for future research. Collectively, the observations reflect noise more than shapes of archaeological distributions. Thus, they should not be used to inform future research.

It is precisely here that the lack of methodological explicitness and the non-problem-specific consumption of cookbook method become an obstacle. Without having fully specified those kinds of information that are vital for the process of estimation, we simply cannot evaluate our observations for their predictive potential. Even comparison between different sets of observations is difficult, if not entirely useless, where we cannot satisfactorily resolve whether the observed differences pertain to the shapes of archaeological distributions, or to minor, major, or entirely uncontrollable differences in method, quality control, and reporting. Certainly, nothing useful will emerge if we merely take the given observations at face value and liberally extrapolate from them in terms of what should be done elsewhere.

This applies as well, for several reasons, where investigators have been exceedingly explicit about how, where, and why a given set of observations was obtained. On the one hand, the inherited sampling design, systematic aligned sampling on a square grid, does not lend itself to the process of statistical estimation. As usually applied, with a single randomly generated starting point, no confidence limits can be placed around population estimates (cf. Cochran 1977). Short of that, one is confined to statements about the smallest size clusters that would have been intercepted with certainty, given the grid size, or about the chance of having missed clusters of smaller size (which, as we have shown, is rather large, given modal grid distances). On the other hand, modal sampling fractions and sample sizes are so small that, even where one could develop confidence intervals around population estimates, they would be so large that they would offer neither help in specifying distributions nor in predicting from observations.

Lacking a means to adequately extrapolate and predict from what has been observed, we are virtually forced to treat the observations at face value. That is, all we know, and all we know reasonably robustly, is that resources of given kinds, counts, and description have been discovered in the given test units. To distill from this information something that is im-

plied for the nonobserved parts of the given research area, or for research areas elsewhere, will be a major research task for the coming generation of archaeologists.

This task should be guided by three considerations:

1. Where observations of archaeological distributions are accurately provenienced, we should take a very careful look at their context, in terms of local geomorphology, soil, fauna, flora, microclimate, access to resources and transportation corridors, etc. These factors of association, over the whole of Massachusetts, and by time period and subregion, should all be part of that polythetic set which helps in anticipating the distribution of similar observations elsewhere (e.g., Dincauze 1978; Williams *et al.* 1973). The goal should not just be to replicate the same kinds of observations. Rather, it should be an attempt to prove the anticipation wrong, refine it, extend it, and integrate it with more behavioral sets of predictors, and, thus, to increase our scientific control over archaeological distributions. Otherwise, nothing new will be learned.

2. Where investigators merely blow up their set of observations, in a statistically and logically uncontrolled fashion, into statements about what has not been observed, an untested hypothesis is the result. This hypothesis tends to be a weak one, given the previously documented shortcomings. In this case, future investigators will gain by eliminating what is uncontrolled superstructure and going back to step 1. Alternatively, we can derive implications from the generalizations for comparable areas elsewhere that have been studied previously, with well-reported, nonbiasing sets of observations, to see if they hold water or if alternative hypotheses would predict the observed distributions as well, if not better. This would accomplish the same result as the method mentioned in the first consideration, but without fieldwork.

3. Predictively, we can rely on the extant observations only for information regarding the largest, densest, and most highly visible aspects of clusters within archaeological distributions. Even for these, many research strategies have been inadequate in their intensity and explicitness. The same aspects of distributions, of course, can also be anticipated from other evidence.

## CONCLUDING SUGGESTION

The distribution of any given variable within the opaque project volume can normally be fully specified only after the complete examination of its three dimensions. This would obviously defeat the purposes of contracts,

and nowhere near sufficient time, money, nor personnel are available to even approximate this task. Clearly, the distributions of archaeological variables can only be partially observed and, more importantly, only be imperfectly specified at best. This places the burden of proof on investigators to demonstrate that

1. They can obtain sufficient observational detail on the project volume that they can infer the distribution of resources within acceptable limits of accuracy, precision, and reproducibility. This is a question of prospecting, sampling, and statistical estimation, in the context of relevant behavioral and environmental information. Given the peculiar conditions in the eastern woodlands of Massachusetts, it is highly unlikely that investigators will ever be able to increase sample sizes, sampling fractions, or test unit sizes to a level of intensity that would generate observations which reflect significantly more than noise.

2. Their knowledge of distributions and all relevant information on their shape in general, and on the given area specifically, is sufficient to approximate the shape of the distribution of interest prior to fieldwork. In this case, the investigators need to demonstrate that they will carefully evaluate the implications of their approximation against a necessary and sufficient number of different aspects of the project distribution to see if the approximation is not a false one. This is a question of strong inference and hypothesis testing. Since hypothesis testing merely calls for logically necessary observations rather than a sample of observations approaching utopian size, it appears that it offers the only path out of the trees and into the forest. While hypothesis testing will make the work of the sponsors of research on archaeological distributions more difficult, it will help them in discharging their stewardship of archaeological resources more conscientiously. More importantly, it will contribute robust knowledge about archaeological distributions and their generating functions, thus adding to anthropological theory rather than to the collection of meaningless numbers.

## ACKNOWLEDGMENTS

This chapter is dedicated to Jude and Woozl who silently suffered while I was sitting in front of yellow pads and typewriters in my office. The paper could not have been completed without the active and passive cooperation of the members of my department who, by staying away from my office, freed enough time so that I could get it all down on paper. The research reported here was my contribution to a team project. Without the painstaking, laborious, thankless, substantial, and substantive work of my collaborators, Dena Dincauze, Robert Hasenstab, and David Lacy, this chapter would not have been possible. Our research was

performed under a Survey and Planning Grant from the Massachusetts Historical Commission. Although I would like to, I cannot blame any of the errors of fact and interpretation on anyone but myself. As usual, the stimulating work environment provided by my colleagues, by the staff, and by the students of the Department of Anthropology at the University of Massachusetts significantly added to the positive attributes of this paper. The paper title was developed in collaboration with Ruthie Kuchyt, Barbara McGlynn, and C. J. Massey, who translated my Germanism into an acceptable English idiom. Where my style is smooth, credit is due to Maura Keene who edited this chapter and prepared its bibliography.

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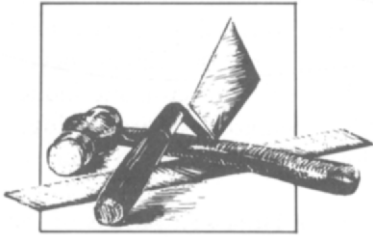
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# 4

## Twigs, Branches, Trees, and Forests: Problems of Scale in Lithic Analysis

JOHN R. CROSS

Over the past two decades, lithic analysts have provided increasingly detailed reconstructions of stone tool manufacture and use. Drawing on the literature from brittle fracture (Cotterell and Kamminga 1979; Faulkner 1972; Speth 1972), experimental replication (Bonnichsen 1977; Callahan 1979; Crabtree 1972), use-wear formation (Keeley 1974, 1980; Odell 1979; Semenov 1964), core refitting (Cahen *et al.* 1979; Frison 1968), and ethnoarchaeology (Ebert 1979; Gould 1977; Hayden 1977; White and Thomas 1972), archaeologists have shifted the emphasis from assemblages to activity-specific tool kits, tools, and portions of tools involved in task performance. Fine-grained interpretation has reached the point at which archaeologists may now talk of isolating individual tool makers and users in the archaeological record on the basis of distinctive flake-scar patterns (Gunn 1975, 1977; McGhee 1979, 1980; Muto *et al.* 1976), debitage characteristics (Johnson 1977), or patterned traces of use-wear (Kay 1977).

Developing the methodological capability for addressing questions of

anthropological relevance in no way guarantees that researchers will ask those questions. In spite of the reorientations brought about by the New Archaeology, lithic studies have retained a strongly empirical and generally atheoretical character, maintaining an emphasis on reconstruction—of tool function, activity areas, and site function. The lack of integration with general archaeological and anthropological goals can be attributed to the specialization that characterizes lithic studies (see Dunnell 1980:466; Schiffer 1979:15–16).

Current emphasis on reconstruction is an extension of an interest in function that developed in the 1960s in reaction to culture-historical approaches. Landmark studies by Semenov (1964), Binford and Binford (1966), Frison (1968) and Wilmsen (1968, 1970) reflect the shift. The popularity of functional arguments in lithic studies may be due to the fact that function is amenable to quantifiable and replicable experimentation. Hypotheses regarding the mechanical efficiencies of different tool forms or raw materials can be tested experimentally (e.g., Keller 1966; Saraydar and Shimada 1971); the social dimensions of technological subsystems are less easily modeled. The development of analytical techniques, such as experimental replication, experimental use, and scanning electron microscopy, has prompted archaeologists to attempt reconstructions of behavior at ever-finer scales. Unfortunately the degree of refinement in observation and measurement exceeds our present abilities to make sense of the observed variation or to deal effectively with the social context of tool production, use, and discard.

The trend towards greater refinement in observation and interpretation is especially reflected by the growing interest in idiosyncratic behavior as it may be expressed in archaeological materials (Hill and Gunn 1977). This paper will evaluate attempts to isolate individual artisans through lithic artifacts and debitage, and offer some suggestions for alternative directions in lithic research. In the context of the discussion that follows, *idiosyncratic* will be treated as synonymous with “peculiar to one individual.”

## THE INDIVIDUAL IN PREHISTORY

### Rationale

Since no two objects are alike if examined at a sufficiently refined scale (Spaulding 1977:4), archaeologists must decide which kind of variation is relevant to the problem or problems under study. Because the individual is the locus of tool manufacture and use (Bonnichsen 1977:58; F. Plog 1977:13–14; S. Plog 1980:116), variation or patterning originate at the

individual level. Thus Hill and Gunn suggest that the individual serves as the starting point for analysis:

We must be able to distinguish and isolate the particular kind of variability that is of immediate relevance to us in answering a given research question or testing a given hypothesis. Even if we are not particularly interested in studying the behavior of individuals, we must be able to control for this variability in order to be certain that we are in fact studying that variability in which we are directly interested [1977:4].

In such studies, residual variation is thought to reflect more inclusive levels of sociocultural integration, such as the family, sodality, or ethnic group. The underlying assumption is that there are nested levels of social distance represented by the degree of similarity among artifacts and among assemblages (Deetz 1967:108). Shared ideas about form or technique result in similar artifact form and assemblage composition; close similarities reflect intense social interaction, or spatial or temporal proximity.

The stated goal of isolating individual craftsmen is to aid in the development of theories of form and style, theories necessary to inform studies of intra- and intergroup dynamics (Hill 1978:246; Hill and Gunn 1977:1; McGhee 1980:444; Plog 1977:16–17). It is argued that through such efforts, archaeologists will have a technique by which they can study craft specialization, exchange, population movements, burial relationships, residence patterns and units, the degree of intra-individual variation in artifact manufacture and use, and social organization in general (Hill 1977:57–60).

Translating these arguments into the study of lithic technology requires an understanding of raw material properties and the range of strategies employed to control rock fracture. From experimental research, lithic technologists have become increasingly aware of the complexity of the flint-knapping process. Control over fracture involves the simultaneous manipulation of such variables as platform preparation, platform angle, impact system, support position, and the force, angle, and placement of blow. Although there has been some success in specifying the relationships among these variables (Bonnichsen 1977; Callahan 1979; Del Bene 1981), isolating specific manufacturing inputs on prehistoric specimens has proven difficult. For each flake removal, an individual artisan interacts with a piece of stone (with its own idiosyncratic properties) in a manner that combines muscular control, experience, and goals. Even greater control over form would be expected for ground-stone tools, and a greater range of muscular control would be allowable without risk of breakage. With pecking and grinding, shape is achieved by the removal of smaller microstructural units (to borrow Bonnichsen's terminology).

Two perspectives on the individual authorship of stone tools have emerged from this background, one emphasizing the goals of the artisan (mental template), the other stressing muscular control and the abilities of individuals to give their ideas material existence.

### Motor Behavior

Archaeologists have long recognized motor skill as a source of artifact variation, but for the most part have treated it as “noise”, since non-transmitted behavior had no diagnostic value in time–space systematics (Deetz 1967:108; Goodman 1944:415; Krieger 1944:272). However, with a change in emphasis from chronology to function, material culture carried new potential for interpretation that extended beyond its role as a time–space marker.

The potentially distinctive flake-scar pattern generated by individual motor behavior forms the basis of Gunn’s (1975, 1977) assertion that individuals can be identified in the archaeological record. Such variation reflects subtle and characteristic patterning which can be neither taught nor learned. As Hill (1978:245–246) explains, the target level for analysis of variation is below that of the design element (which can be imitated) and above that which can be expected in the work of one individual. Based on his correspondence with Don Crabtree, Gunn develops a list of potentially useful indicators for idiosyncratic behavior in lithic assemblages: several variables are referable to muscular control (e.g., accuracy, bulbs of percussion) whereas others relate to individual preferences in technique (e.g., platform preparation, striking angle).

To demonstrate the feasibility of identifying individual flintknappers archaeologically, Gunn (1975) focuses on flake-scar orientations. Using a series of bifaces made by modern knappers and patterned after a single specimen, and bifaces from a cache thought to be the work of a single prehistoric knapper, Gunn was able to attribute all but 1 of the 60 flake-scar patterns to the proper knapper through a combination of principal components and discriminant function analyses. Not unexpectedly, the prehistoric knapper showed the tightest clustering, thus offering empirical support for Crabtree’s hypothesis (cited in Gunn 1977:175–176) that modern flintknappers have a broader repertoire than had their prehistoric counterparts, but have less familiarity with particular raw materials or techniques.

McGhee (1979:107, 1980:446) uses a different set of criteria, ranging from spatial distributions of artifacts to motor behavior (“handedness” of burins, fineness versus crudeness of execution) to choices among technological alternatives (raw material selection, size, edge grinding, type of retouch)

in order to support his identification of individual artisans within the Arctic Small Tool tradition. McGhee assumes that collections from archaeological features represent the work of single artisans, although if the range of variation in artifact form is “too great”, he infers the presence of additional individuals. In contrast with Crabtree’s observation that flakes contain more information about manufacturing techniques and motor skills than finished artifacts (in Gunn 1975:39), McGhee’s emphasis is entirely on artifacts. Debitage is excluded along with burin spalls and microblades because they are “too constrained by their process of manufacture to allow useful study in terms of individual style” (1979:107).

Johnson’s analysis of quarry debitage examines both individual motor patterns and culturally conditioned lithic reduction strategies to identify individual knappers. The next phase of Johnson’s analysis of the Aguas Verdes assemblages necessitates separating technological constraints from characteristic behavior referable to a single individual:

If the debitage I produce in duplicating an Aguas Verdes point is statistically identical to that produced by the Aguas Verdes knapper(s) who worked at RAnL 87XX, then it will have to be concluded that lithic debitage is of marginal use in identifying prehistoric individuals and that the debris from the process of making Aguas Verdes points is technologically determined to a great extent [1977:225].

### Criticisms of the Motor Behavior Approach

In reviewing studies relying in part or entirely on differences in individual motor patterns to identify prehistoric artisans, a number of problems emerge. First, assumptions have often been accepted uncritically. Second, analogies are often applied without regard to their validity. Finally, we have frequently failed to recognize that similarities in assemblages can be the product of *different* behaviors.

McGhee’s case for individual stylistic variability among Arctic Small Tool tradition flintknappers is flawed by the absence of uniform criteria for identifying artisans. Priority is usually (though not always) given to interpreting the tools associated with a single feature as the work of a single knapper. At other times, McGhee infers individual craftsmanship from formal and technological criteria (a mix of motor habits and conscious selection from among technological alternatives), fineness of execution, “handedness” of burins, the use of a particular raw material, and volume of tool production. His results are preconditioned by a priori assumptions about the range of variation within an individual’s work, and hence are neither replicable nor testable.

One of the central assumptions of the motor behavior perspective is that an individual's motor habits remain constant throughout the lifetime of that individual, or at least that the changes are sufficiently slight to allow correct attribution of workmanship (Hill 1978:253). Hill's test of this assumption is on a handwriting sample taken from the letters of four novelists; the extension of his "no change" assessment to other media is a statement of faith. In looking at lithic technology, several factors come to mind which might introduce variation over the course of an individual's lifetime: (1) developmental and degenerative changes in biomechanical relationships that would affect the accuracy and force of blows (Muto *et al.* 1976:275 suggest arthritis as a possibility); (2) trauma or fatigue might be expected to affect performance on a day-to-day basis; and (3) the experience level of the knapper, since muscular habits are modulated by the feedback of success or failure. It remains to be seen whether initial attempts at biface manufacture could be attributed correctly to the same individual who had since developed into an accomplished flintknapper (see Callahan 1979:35, 37-38).

Gunn's study takes up the issue of analogy by examining the relationship between modern flintknappers and their prehistoric counterparts. Modern knappers have a broad understanding of techniques and raw materials, but for a given material and the techniques suited to working it, the prehistoric artisan may have a greater depth of understanding. A group of prehistoric knappers, coming from the same learning context and interacting with one another, would overlap considerably in the decisions made during manufacture. As a result, the flake-scar patterns for a given artisan or group of prehistoric flintknappers would cluster quite tightly, more so than would be expected for a group of experimental lithic technologists. As I have suggested earlier, separating motor behavior from culturally conditioned strategies of stone working is not an easy task. This is not to say that idiosyncratic motor habits cannot be detected in a sample of prehistoric artifacts; however, tight clustering alone does not guarantee the isolation of individuals as Gunn (1975:38) and Johnson (1977) have argued. Gunn's work opens the door for studying the nature of social organization among modern flintknappers, an area deserving attention in light of the recent emphasis on experimental analogues.

An alternative explanation for the clustering measured by Gunn is that he is recording patterns produced by different impactors rather than by different knappers (T. Del Bene, personal communication 1980). Hammerstones or billets of a given mass and resilience tend to generate flakes of characteristic proportions, a factor which may mask individual motor patterns. The situation may be analogous to the San José pottery painters observed by Hardin (1977:135): an individual's brushes produced diagnostic strokes, not the artisan's motor behavior *per se*.

Johnson's use of experimental replication exemplifies some of the pit-

falls associated with assuming a one-to-one correspondence between experimental and prehistoric behavior when similar results are achieved. Johnson proposes a test of the utility of the idiosyncratic behavior concept for explaining Aguas Verdes debitage which hinges on her ability to produce “statistically identical” debitage in the replication of Aguas Verdes points. One of the major lessons learned from lithic experimentation over the years has been that similar results can be achieved by a number of different means (Callahan 1979:3). Similar flake morphology can be generated by modifying the striking angle, or the force of the blow, or by changing impactors. Until input variables for tool manufacture can be related to specific results in flake morphology, the question of muscular control versus technology cannot be resolved satisfactorily.

Resharpener or stage production of tools by different individuals may render many of the arguments for idiosyncratic motor behavior useless. Gunn’s use of flake-scar orientation is particularly susceptible on this point. Multiple authorship of lithic artifacts could confuse the analyst by showing evidence of several “hands”. Callahan (1979:3) has suggested that the initial steps of biface manufacture condition the morphology of the final product; depending on the nature of the blanks or preforms involved, two artifacts of the same general form and made by the same knapper might easily be attributed to different artisans on the basis of flake-scar orientation. Gunn (1975, 1977) and Muto *et al.* (1976) avoid the problem somewhat by focusing on artifacts from caches and burials, which show little evidence of use and, although they may represent stage manufacture by several individuals, are likely candidates for having passed through the same manufacturing processes. An analysis that attempts to isolate individual craftsmen will have to consider the organization of lithic manufacturing subsystems and the use lives of artifacts under study (Binford 1979:20–21).

## Mental Templates

The template and motor behavior perspectives can be thought of as complementary rather than as mutually exclusive. The differences are in emphasis, the former stressing ideas about artifact form that exist in the mind of the maker, the latter emphasizing the fact that different individuals have different abilities to objectify their ideas.

Although Rouse (1939) and Krieger (1944) are recognized as early influences on the development of the mental template concept, Deetz has given the term its most frequently cited definition:

The idea of the proper form of an object exists in the mind of the maker, and when this idea is expressed in tangible form, an artifact results. The idea is the mental template from which the craftsman makes the object. The form of an

artifact is a close approximation of this template, and variations in a group of similar objects reflect variations in the ideas which produce them [1967:43–44].

According to Deetz's model, the individual is one level in a nested set of levels that reflect degrees of sociocultural integration. Some ideas or experiences are truly unique and are shared with no other individual. Other ideas about form or technique are shared with family members or more broadly within the community. Still other ideas are held in common by members of several interacting communities. It follows from the template argument that the degree of similarity among artifacts or assemblages can be taken as a measure of the degree of social interaction among groups (e.g., Kay 1975).

The template argument examines change within a normative framework (Binford 1972). Since individuals are responsible for tool manufacture and use, all changes originate in the minds of individuals. Shifts in perceived needs or aesthetic norms (Gallus 1977:135) and diffusion are seen as the major causes of change.

The template argument has many variants and goes by many names (cognitive archaeology [Bonnichsen 1977]; folk taxonomy, organic typology [Gallus 1977:134]). For example, Bonnichsen has criticized Deetz for setting "the impossible objective of attempting to define emic categories (mental templates) rather than etic categories which are far more subject to investigation in prehistory" (1977:54). Bonnichsen stresses the decision-making aspects of lithic technology, and frames his argument from the perspective of cognitive anthropology. In practice, the approach is similar to Gunn's (1975:35–36), which links goals to performance through feedback loops. Procedural rules for lithic reduction are modified according to the artisan's success in controlling fracture. Goals and expectations change constantly throughout the manufacturing process in response to errors or unpredictable fractures. Bonnichsen's position should be seen as an amendment to Deetz rather than a wholesale replacement of it, since both seek to explain persistence or change in artifact form through an understanding of the needs, goals, memories, and experiences of the prehistoric artisan (Bonnichsen 1977:57).

### Criticisms of the Mental Template Concept

While heuristically useful as a means of pointing out the decision-making elements in any technological tradition, the template concept is of limited value in an operational sense. It is very difficult to distinguish variations contributed by unambitious conceptualization of form (mental template) versus poor execution (motor behavior). The picture is further com-

plicated by the realization that many of the artifacts recovered by an archaeologist were rejected during the manufacturing process or were modified to the point that they were no longer functional. Few lithic artifacts will be “close approximations” of the mental templates involved in their production at the time when they are ultimately committed to an archaeological context.

The template perspective reduces mechanisms of change to the vagaries of the human mind; change occurs because ideas about form or technique change or because personnel change. Explanation beyond this point is felt to be unnecessary, since all behavior is the product of unique minds in unique historical contexts (Gallus 1977:134). Plog has pointed out the circularity inherent to many attempts at paleopsychology:

To attempt to infer different motivations or perceptions from artifacts and then to use variation in motivation and perception to explain artifactual variability is to become enmeshed in a hopelessly circular chain of argument [1977:17].

Binford has criticized the mental template perspective for generating unfounded expectations about the nature and rate of change in material culture (1972:115–119). He specifically challenges the notions that changes in artifact form should be gradual, that new forms develop from earlier ones, and that similarities among assemblages can be taken as a measure of the extent to which ideas are shared within and among groups. Thus, changes that do not approximate the familiar “battleship curves” of seriation cannot be accommodated easily by the mental template concept.

The process of template replication generationally or among contemporary artisans is often treated as a cultural analogue of biological evolution (Hole and Heizer 1973:78–79; Keene, this volume). Unrealistic models of change can follow from equating innovations and mutations, types and species, variables and genes, and attribute combination and hybridization through gene flow. A review of the difficulties in treating temporal trends in artifact form as biological evolution is beyond the scope of this paper; I raise the point here to indicate one way in which the template perspective has affected interpretation.

### General Comments on the Individual in Prehistory

Conceptually, both the motor behavior and mental template approaches are hampered by the assumption that the documentation of particulars will generate theory. Hill and Gunn (1977:4) suggest that we can arrive at a theory of form and a theory of style by mapping the interactive behavior of prehistoric artisans as individuals. Advocates of the template

perspective argue that we can explain the behavioral products of material culture variation only by focusing on individual mental templates (Gallus 1977:135). I would argue that the reconstructionist stance (i.e., building from the sum of component parts) is often insufficient and almost always inefficient for generating theory.

With both approaches to idiosyncratic behavior, there is a tendency to substitute reconstruction for explanation. Reconstruction is related to description (Dunnell 1971:15) and expresses the relationships among objects or empirical observations. Explanation is linked to definition and expresses the relationships among variables. Context-specific interpretation lacks the larger generality characteristic of explanation. At best, with a reconstruction of actual behavior, archaeologists could contribute to description, not to explanation.

Researchers dealing with idiosyncratic behavior enter the technological subsystem at the point of maximum noise (i.e., maximum variability). At the level of the individual artifact, errors in knapping or flaws in the raw material have a significant impact on form. Since all variables of form can be expressed as the unique interaction of an individual with circumstances, behavioral inputs, and raw material, there is some doubt that there would be any residual variation in form once the behavior of the individual was factored out. If one follows the suggestion of Hill and Gunn (1977:4) that isolating individual variation is a necessary first step in understanding the dynamics of artifact form, then there would be little or no variation in form that could be attributed to greater levels of sociocultural integration than the individual. Analytically, archaeologists would be faced with the uniqueness of each item in their collections. The template argument is even more frustrating on this point, for not only are researchers faced with unique products, but with unique minds behind the products.

In applying idiosyncratic behavior models to archaeological questions, other problems emerge. Redman (1977:42) notes the enormous expenditure of effort necessary to demonstrate conclusively that three rather than four individuals were represented by a given assemblage. Demonstrating or falsifying any argument about prehistoric individuals might consume a tremendous amount of time and effort with minimal returns. Aside from personalizing prehistory through identifying brief moments or actions (see Cahen and Keeley 1980), the search for individuals in the archaeological record remains a questionable expenditure of time and effort in the absence of well-defined problems. Redman offers the "analytical individual" as an alternative unit of study. He acknowledges the presence of small-scale variation, but sidesteps the thorny issue of a one-to-one correspondence of his units with actual individuals.

Finally, the chronological issues are themselves formidable obstacles to

studies of idiosyncratic behavior in the archaeological record. Unless stratigraphic control is excellent, archaeologists may have difficulty demonstrating the contemporaneity of any two individuals, except within the unacceptably broad range provided by radiocarbon dating. Intersite comparisons pose even greater problems in this regard.

## ALTERNATIVE DIRECTIONS FOR LITHIC STUDIES

Recognizing that it is always easier to criticize an innovative approach than to offer alternatives, I shall suggest a number of problem areas toward which lithic technologists, archaeologists and anthropologists might channel their energies in the next few years. Many of the topics represent challenges to our assumptions about lithic technology and prehistoric technologies in general.

### Organization of Lithic Technologies

Archaeologists tend to assume that technological traditions associated with a given subsistence level or within the same culture-historical or linguistic tradition have the same organizational structure. Do all members of a group engage in technological activity (in this case, stone tool manufacture and use), or are there restrictions on access to raw materials or in access to information on how to make stone tools? If stone tool manufacture is restricted to a few individuals in a group, then the implications for mechanisms and rates of change are different than in situations characterized by generalized knowledge of how to work stone. Specialization, combined with distribution networks for finished or roughed-out artifacts, may result in rapid and widespread changes in artifact form. We might also expect greater standardization of form and technique in situations where specialists are involved (Sheets 1974:2–4). In cases where flintknapping is widely practiced, the expectations are different; individuals with limited or infrequent experience would generate localized tool traditions typified by a high degree of internal variation in form and technique.

Within any group, there should be different skill levels represented, ranging from the novice to the expert. What biases do we introduce by assuming that the recovered artifacts represent the work of only skilled craftsmen? Experts may be responsible for producing most of the artifacts while generating minimal debitage; novices may be overwhelmingly represented by debitage and rejects, but only occasionally by finished artifacts. Production failures, frequent hinge fractures, and shattered striking plat-

forms are often taken as measures of inexperience (Johnson 1979:27; Sheets 1974:2–4). The purpose of this example is not to suggest that we should isolate “good” or “bad”, “expert” or “novice” knappers, but to point out that a measurement of central tendency for both debitage and finished tools may be measuring the output of different segments of the population.

It is unlikely that the ethnographic record will provide all of the answers to these and other questions about lithic technology. In any ethnographic sample, lithic technology has either been abandoned in favor of metal (e.g., White *et al.* 1977:381), or is maintained as an expedient solution to problems (e.g., Gould 1977; Hayden 1977). There are few if any ethnographic analogues for lithic subsystems that involve standardization of form or effort expenditure in excess of levels necessary for task performance. As Wobst (1978:303) has indicated, there is no reason to assume that the full range of options available to prehistoric groups is represented by a modern sample.

The ethnographic record, although sparse in examples of specialized tool manufacture, does provide insights into what Binford has called the “situational flexibility” of technology (1979:20–21). The importance of form is often subordinated to the presence of a desirable edge angle or flake size in expedient lithic technologies (White and Thomas 1972:286). Binford raises the question of how technologies are organized to cope with seasonal access to raw materials, transportation costs for artifacts, the use life of artifacts, and how environmental uncertainty is reflected in curated versus expedient tool kits. Of these, only use life has received much attention from archaeologists (see Gould 1977:164–167; Hayden 1977:182 however). The interaction of practical considerations such as transportation costs or seasonal access to raw materials and the social organization of tool manufacture is currently an unexplored area.

### **Articulation of Lithic Technology with the Social and Natural Environment**

Temporal variation in artifact form or technique of manufacture is occasionally invoked as a cause of changes in the social environment or as a consequence of changes in the natural environment. Turnbaugh’s (1975) discussion of Late Archaic culture history in the Northeast provides a case in point. Turnbaugh suggests that climatic changes circa 4000 B.P. brought about a warming of Atlantic coastal waters, which caused temperature-dependent fish-runs to take place earlier in the year along the eastern seaboard. Groups technologically well-adapted to exploit the fish runs expanded northward at the expense of resident populations. Archaeologists

attribute this expansion to the use of broad-bladed bifaces that could not easily be dislodged by impaled fish. The competitive advantage bestowed by the broadpoints was claimed to be sufficiently great that it allowed technological replacement in some areas and population replacement in others.

Leaving aside particular criticisms of Turnbaugh's paper, the confusion over causal linkages may be due to the fact that archaeologists pay lip service to systemic approaches. Drawing double-ended arrows between lithic technology and other components of an operating cultural system (environmental matrix, social organization, ideology) does little to enhance our understanding of *how* lithic technology articulates with the rest of the system. Changes in one component of the system may have a negligible effect on other components, a delayed impact, or an exaggerated effect. The nature of the links between and among components and the extent to which perturbations at one level of the system are buffered at lower organizational levels (van der Leeuw 1981) will determine the systemic response to change.

In many instances, archaeologists may be guilty of assigning causal status to what is only a correlation. Because lithic materials are a common and durable class of material culture, they often assume a greater significance in archaeological interpretation than is warranted. In the preceding example, an obvious change in artifact form (to broad-bladed bifaces) was interpreted as contributing to the "Darwinian fitness" (to borrow Dunnell's [1978] definition of function) of a human group. Technology unquestionably has adaptive value; however, slightly better performance of one tool form at a certain task is a poor choice for prime mover. Lithic assemblages, to some extent, may be of only peripheral importance, part of the cultural repertoire that may vary considerably without fitness penalties.

## Style

In studying social change, the examination of style offers a complementary approach to the comparative mechanical efficiencies of lithic artifacts. It would be difficult to find a term in the archaeological literature with as many connotations and definitions as "style". Wobst (1977:317) outlines a number of problems that continue to hinder its application to material culture: (1) it is often a residual category, subsuming variation for which function cannot be inferred (e.g., Close 1978:228–229) (2) it is "unmanageably multidimensional", even within the context of a single study; and (3) style is thought to have no functional value. Style is so nebulous in its usage that it is often discussed without being defined (e.g., Plog 1980). Frequently it is counterposed with function in analysis (Close 1978:224; Meltzer 1981:316), and is used in the same manner as an index fossil—as a

time-space referent devoid of systemic meaning (Dunnell 1978:199; Sackett 1973:320, 1977:370). Dunnell goes so far as to equate style with stochastic variation in form and with forms "that do not have detectable selective values" (1978:199). Jelinek (1976:20) adopts a more moderate view which holds that style and function are not analytically separable dimensions of artifact variation (i.e., there are no universally applicable ways to identify any one variable as "stylistic" or "functional"). Jelinek (1976:32) and others (e.g., Binford 1979:21) equate style with "preference" or "selection from among functionally equivalent design alternatives".

The definition of style used in this paper differs from several of those just mentioned and follows Wobst (1977) and Conkey (1978) in using style as the designator of the information-carrying content of material culture. Drawing on general systems theory, all systems are involved in the processing of matter, energy and information (Miller 1965:193). Information is used here to include structure (the tendency to resist entropy), in addition to the colloquial definition of facts, data, or news. Function deals with those elements of artifact variability relating to processing matter. Processing energy involves both function and style, since energy can be released through processing matter and is controlled by processing information. In its information-carrying capacity, style "functions" by opening communication channels to messages dealing with group affiliation, ownership, emotional state, rank, status, and so forth (Wobst 1977:323). The target group for stylistic behavior is intermediate in social distance between those so close that messages could be more effectively transmitted through another mode, and those too distant socially to understand the message. If the stylistic mode is being used for messaging, then archaeologically we might expect a rather sudden standardization of form. Style in material culture is a binary switch; once a class of objects is used to signal, then all objects in that class lose their signalling neutrality and become potential information carriers. The potential for artifact form to carry messages in this way effectively negates Meltzer's (1981:313) assumption that style, function, and shape remain in a constant relationship within a tool class.

Stylistic behavior plays a role in the formation and maintenance of social boundaries (Conkey 1978:79; Hodder 1979; Wobst 1977). Variation in the form of material culture may serve to integrate or differentiate groups. Suitable items for boundary-maintaining behavior must circulate in intergroup interactive contexts. Wobst suggests that hair style and clothing are highly visible at distance and are thus good candidates for carrying messages about group affiliation. Hodder stresses ornaments and clothing, and Conkey favors *art mobilier* as potential transmitters of social information.

Lithic materials may also serve to encode messages, since lithic artifacts

are generally portable and might be expected to be present in boundary-maintaining situations. Although not yet as strongly developed as cases for clothing or hair style, the argument for information transmission in lithic materials deserves some consideration. With lithic artifacts, the features most likely to identify messages would be form, color and raw material. Wilmsen and Roberts (1978:27) and Voss (1977) suggest that bifaces, because of their relatively high effort investment and because shape is effectively controlled by the removal of many flakes, might serve as possible vehicles for information transmission.

Although archaeologists may not be able to predict the configuration of formal change by using the stylistic approach (hence Dunnell's perception of stylistic change as stochastic), we should be able to predict the circumstances under which stylistic behavior will become a viable alternative for prehistoric groups. We need not be able to predict a shift to stemmed projectile points from triangular ones, but we may be able to identify system-states when stylistic changes are likely. It is important to keep in mind that the relationships between and among style, form, and function are not static, even within a given artifact class. It is also apparent that style and function cannot be understood in isolation, but only make sense when considered in the context of a research problem.

## SUMMARY AND CONCLUSIONS

Throughout this discussion, there has been an overriding concern about the relationship between lithic studies and anthropological archaeology. Although arbitrarily limited to lithic materials, many of the suggestions offered here apply to all technological subsystems. It is from that general perspective that we should approach lithic studies, not from the atomized view provided by refined methodology.

Because of the preferential preservation of stone over organic materials at many sites, lithic materials constitute a common denominator for inter- and intrasite comparison. Regardless of overall research goals, an archaeologist who opts for excavation will encounter lithic materials at most prehistoric sites. The standards of the profession demand that the recovered material be analyzed, regardless of the research problem under study or the appropriateness of lithic artifacts for addressing that problem. The need to "do something" with excavated tools and debitage, coupled with inadequate attention to problem formulation, can lead to methodological myopia and a vast literature of uneven quality.

Given the strong tendency for lithic studies to be assemblage-specific (i.e., the collection to be analyzed determines the research questions and

strategies), the fact that methodology has outpaced theory is not surprising. As Saitta (this volume) has noted, “. . . even those archaeologists who have furnished insights into the disciplinary trends of the last twenty years tend to view an appropriate question as one that can be answered with available data”. It may be axiomatic that a failure to achieve explanation results in the collection of more fine-tuned data, not to theory-building in most cases. The temptation is great to allow an increasingly sophisticated methodology to supply as much detail as possible. One must ask if discovering the function or functions of a particular artifact through lengthy use–wear analysis constitutes a worthwhile expenditure of research effort when so many larger issues remain unresolved.

Currently, lithic analysis seeks fine-grained solutions to coarse-grained questions. This results in a high-resolution image of particular behaviors in a larger context of poor chronological resolution. We cannot yet distinguish among cases in which similarity of artifact form represents temporal proximity, spatial proximity, intense social interaction, functional equivalence, constraints imposed by common techniques, technology, raw material, or participation in information transmission in which similar messages are being sent. Detail without matrix is of limited value. It is as though we were examining an elephant under a microscope. The sum of a series of views would give us detail, but in no way could we recognize the elephant as such. In the context of this argument, Leslie White’s criticism of Boasian particularism has special relevance:

Not only did he fail to see the forest for the trees, he could scarcely see the trees for the branches, or the branches for the twigs. And no two twigs were alike . . . (1946:78).

## ACKNOWLEDGMENTS

I am particularly indebted to Katharine Kerr, Ann Magennis, and Dean Saitta for their considerable advice and support during the writing of this paper. Insightful comments on earlier drafts of the paper by Terry Del Bene, Dena Dincauze, Bill Fawcett, Joan Gero, Lars Johansson, Dave Lacy, Roy Larick, John Pfeiffer, Dolores Root, John Speth, Bill Starna, Doug Sutton, Ed Wilmsen, and Martin Wobst are gratefully acknowledged. I am solely responsible for any flaws that persist.

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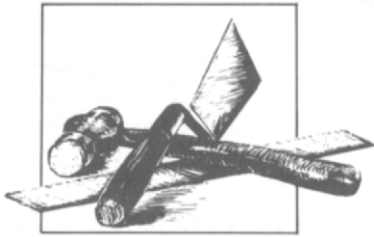
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# 5

## Pots as Tools

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The analysis of pottery plays a leading part in the reconstruction of culture histories in archaeology. Our analytical sophistication has grown significantly over the decades, as has our understanding of the technology of pottery production. Generally overlooked in the analysis of pottery, however, is the fact that most pots were not made simply to be shaped, tempered, and decorated. Most pots are implements; they are made to be used as containers. Many of the attributes of pottery routinely recorded from sherds for purposes of culture-historical classification are, in fact, evidence of the techniques used by potters to achieve particular characteristics of utility in the finished vessels. When examined in an appropriate theoretical framework, these attributes inform us about variation in vessel use, providing complementary evidence of variation, for example, in practices of food preparation and storage and other aspects of prehistoric behavior.

The purpose of this chapter is to advocate a change in the way in which we routinely approach the analysis of pottery. More broadly, however, this chapter concerns two all-too-common conditions in archaeology: (1) a lack of integration of analytical methods with interpretive theory, and (2) a sophistication with measurement that runs ahead of our sense of what the

measured variation means (Deetz 1968:48; Marquardt 1978:295–302; Thomas 1978, 1980; Whallon 1972:32).

## BACKGROUND

Pottery is an archaeologist's delight. Its brittleness guarantees frequent breakage and disposal; its crystalline structure virtually guarantees preservation. Attributes of vessel paste composition, morphology, manufacturing technique, decoration, and place of disposal can be recorded as easily from fragments as from whole vessels. Further, whereas paste composition may vary as a result of both natural and cultural factors, all other attributes mentioned here vary entirely as a result of cultural factors alone. The archaeologist, consequently, has found pottery a seemingly ideal tool for culture-historical systematics.

The archaeological importance of pottery, of course, goes well beyond its use in culture-historical systematics. As the products of manufacturing systems, pots bear evidence of how they were manufactured. As parts of the visual environment, they often acquire style or decoration and so bear evidence of aesthetics and the organization of social networks. As manufactured *goods*, they often serve as media of exchange or of the marketplace and so can reveal some of the characteristics of economic networks.

But pots also are tools. Their morphology and composition, and to a certain extent, their decoration as well, are in fact constrained by their intended contexts and conditions of use. Conversely, what may be termed the *mechanical performance characteristics* of a completed vessel, affecting its effectiveness for performing particular tasks, are conditioned by the attributes of manufacturing technique, vessel morphology, and paste composition.

Mechanical performance in ceramic vessels embraces three separate aspects of performance:

1. The suitability of a given vessel shape as a container that accepts both the inward and outward flow of some particular material. Performance in this realm is conditioned by the size and shape of both the vessel body and its orifice(s) (Braun 1980a; Smith 1981).

2. The suitability of a given vessel shape for manipulating its contents in some particular manner, for example, storing, transporting, heating or cooling, or controlling both the physical and chemical stability of the contents. Performance in this realm is conditioned not only by body and orifice shape, but also by the presence of different kinds of appendages, and by the composition, cross-sectional shape, and crystalline structure of the vessel walls (Braun present volume; Ericson *et al.* 1972; Smith 1981).

3. The ability of a vessel to withstand the physical stresses entailed in its use, without failure, over a reasonable use life. A vessel that fails during use is, in fact, worse than simply useless. Failure during use involves not only the loss of the vessel, but usually the loss of its contents as well. Vessel failure can also have undesirable effects on the place or facility in which it is being used, and on the people using the vessel at that time. Resistance to failure is conditioned in part by whole vessel shape, but primarily by the composition, cross-sectional shape, and crystalline structure of the vessel walls (Braun 1982, present volume; Rye 1976, 1981; Steponaitis 1980: 28–83).

Attributes of morphology and composition, in turn, affect a vessel's mechanical performance in both positive and negative ways, depending on the vessel's intended use and the specific kinds of stresses developed during both manufacture and use. The potter therefore must establish a compromise in the selection of raw materials and manufacturing techniques to be combined with different vessel forms. This compromise involves the balancing of choices according to their labor and material costs, and the desired vessel life expectancy, relative to the need or demand for the final product (cf. Matson 1965; e.g., van der Leeuw 1976).

The archaeologist, in turn, can benefit from the prehistoric potter's compromises. Variation in mechanically sensitive attributes of morphology and composition indicates variation in the relative importance of the factors conditioning the compromise. In theory, then, these mechanically sensitive attributes, when their mechanical meaning is recognized, provide the archaeologist with the means for explaining ceramic technical variation, rather than just describing it.

The present state of ceramic functional studies in archaeology, however, reveals a need for the integration of four generally disparate directions of archaeological research: archaeometry or archaeological science; chronometric theory; the use of engineering concepts in the analysis of implements; and the treatment of artifact style as visual communicative behavior. Suggestions for a more "functional" approach to ceramic analysis (e.g., Bennett 1943; Binford 1965; Holmes 1903:24–63; Linton 1944; Matson 1965; Rice 1976) are as old as the disparity itself.

## Archaeometry

The archaeological literature abounds with discussions of how to measure ceramic attributes from sherds, ranging from general reference works (e.g., Hodges 1964; Rye 1981; Shepard 1968; Tite 1973) to specific manuals (e.g., Bennett 1976; Colton 1953; March 1934). Considerable sophistication has developed in petrographic methods, trace element analysis, and

crystalline phase analysis (e.g., various chapters in Brill 1971, or almost any issue of *Archaeometry* or *Journal of Archaeological Science*). With the exception of analyses of chemical residues of use (e.g., Condamin *et al.* 1976; Duma 1972), this archaeometric sophistication has been focused almost exclusively on the analysis of manufacturing location and firing procedures for studies of exchange–trade and manufacturing technology (e.g., Fry 1980; Peacock 1970, 1977; van der Leeuw 1976).

In the wake of the long-continuing development of laboratory technique, the profession has acquired an array of routine macro- and microscopic methods for examining, for example, the texture and composition of pastes and their nonplastic inclusions. Archaeologists most frequently apply these methods to the identification of attributes that vary over time or space, allowing him or her conversely to identify the times and regions of vessel origin. The underlying interpretive framework is normative and historical, even when whole-vessel functional categories and geological constraints on raw material availability are recognized (e.g., in the ethnoarchaeological literature, Arnold 1971, 1975, 1978; in the archaeological literature, O'Brien 1972, and MacNeish *et al.* 1970 are just two examples of the hundreds available). However, as will be shown later in this chapter, the laboratory measurements developed for culture-historical purposes are often equally appropriate for the study of ceramic vessel use.

### Chronometric Theory

Chronometric seriations of pottery routinely begin with an attempt to identify types, or, more recently, attributes, which are “primarily temporally sensitive,” for inclusion in the seriation model (Marquardt 1978). All temporally sensitive criteria satisfying various statistical prerequisites are then combined to produce the ordering instrument, a measure of similarity or dissimilarity among objects or lots. However, the selection of ceramic attributes for a seriation model because they are primarily temporally sensitive, without any consideration of why their states are changing over time, defeats one of the ultimate goals of seriation: to produce an ordering whose scale duplicates the temporal relationships among the ordered units on an actual *interval* scale.

Even if particular ceramic attributes vary only unimodally over time, each may be responding to changes in a different set of “environmental” constraints. This caveat unquestionably applies to those attributes that are mechanically sensitive. The mechanical constraints on different ceramic attribute states will vary according to the specific cultural demands placed on vessel performance. The underlying cultural changes will not occur at the

same rate, nor follow the same pattern of variation in their rates of change (Plog 1979). A seriation model that combines attributes sensitive to different cultural constraints therefore cannot be assumed to avoid blurring and distorting the true interval-scale temporal relationships present, even if depositional-contextual variation among the ordered units has been controlled (cf. Marquardt 1978). Ideally, temporal seriation models should rest on a prior selection of attributes according to explanations of their cultural meaning. That is, temporal seriation models ideally should resemble continuous time-series models of change in specific cultural variables (e.g., Hargrave and Braun 1981).

### Engineering Analysis

The use of uniformitarian engineering concepts to interpret implement function in archaeology is not controversial. For example, whereas much of the work on chipped stone tools focuses on manufacturing sequences or on wear patterns, the literature also recognizes the basic engineering principle involved: the concentrating and directing of mechanical force along a point or edge (e.g., Dunnell and Campbell 1977:35–37; Hayden 1979; Keeley 1980; Semenov 1964). By assuming the operation of a principle of least effort, edge shape can be analyzed as a response to the need for a means of effective transmission of forces, an acceptably low cost of manufacture and maintenance, and an acceptably low risk of mechanical failure during use (see also Dunnell 1978; Schiffer 1975; Wright 1977).

As noted earlier, most archaeological work on pottery has also focused on manufacturing sequences. Analyses of wear patterns also exist (e.g., Griffiths 1978). Our ability to analyze ceramic vessel use, through a consideration of the abstract engineering constraints involved, however, is not well developed. Smith (1980, 1981) has begun to explore abstract questions of whole-vessel shape effectiveness (see also Braun 1980a; Ericson *et al.* 1972); van der Leeuw (1976) has explored questions of manufacturing costs; Rye (1976, 1981) and others (Braun 1982; Steponaitis 1980) have begun to explore questions of resistance to mechanical failure. Fortunately, because of the importance of ceramics in modern industry, a large corpus of theory on the mechanical performance of ceramic bodies, particularly on properties of strength and resistance to failure, is already available for use.

It could be argued, in opposition to the use of engineering concepts in the interpretation of pottery, that preindustrial potters would not have been aware of the engineering effects of many of their manufacturing decisions. Ceramic technical variation under such circumstances, in the extreme, would bear little relationship to variation in vessel use.

It would be difficult to support such a contrary argument even in the ethnographic record. Smith (1981; see also Braun 1980a:171–184) summarizes studies indicating considerable cross-cultural regularity in the selection of particular attributes of shape for vessels intended to perform particular kinds of tasks, indicating response to a common set of engineering constraints. Arnold (1971), Rye (1976), Rye and Evans (1976), and many others similarly have documented, among traditional potters, either high levels of awareness of the engineering consequences of their actions, or customary practices that prove to be technologically highly appropriate in their engineering consequences (see also Rye 1981).

The reasons for this kind of cross-cultural consistency are readily identified. Where pottery-making is a domestic craft—where the makers also are the users—the potters would necessarily constantly be exposed to evidence of the success or failure of their vessels in use. DeBoer and Lathrap (1979:128), for example, found breakage rates in Shipibo–Conibo pottery produced by *experienced* potters still sufficient to entail the failure of a projected 25,495 vessels among 18 households over a 100-year span of projection, or an average of approximately 14 vessel failures per household per year. In addition to observing their failures, the potters could constantly evaluate the relative convenience and effectiveness of the vessels in use. Furthermore, communication among potters would also spread information on others' successes and failures.

Where pottery making is a domestic craft, we can then assume a selective process. Pottery techniques that produce vessels that are inefficient as tools, require relatively high labor or material costs, or require relatively frequent replacement, will tend to be avoided in favor of techniques that produce more efficient results, at lower costs, with lower frequencies of replacement. Conservatively, then, materials and shapes used by domestic potters can be expected to be, if not actually finely attuned to all details of intended vessel performance, at least *satisfactory* for the demands placed on them.

Where pottery making is not a domestic craft, as, for example, in market-based economies, the grounds for taking an engineering approach to pottery analysis become less certain. Economic and social considerations may overshadow utilitarian considerations, even in the production of pots for domestic use. Although the demand for marketed pottery presumably will depend on its suitability for use, the quality of the marketed goods may well be affected by factors other than the selective demands of the marketplace alone. Where pottery making is not a domestic craft, then, interpretations of ceramic technical variation from an engineering perspective must control for a much wider variety of nonengineering constraints.

## Style Analysis

In addition to serving as implements and as media of exchange–trade, ceramic vessels are part of the visual environment of human behavior. As a result, their decoration and details of shape carry a communicative effect, and are constrained by the social and symbolic environment of the potter (Conkey 1978, 1980; Friedrich 1970; Graves 1979, 1980; Hodder 1977, 1978, 1979; Plog 1980; Wobst 1977). This set of relationships has two implications for the archaeological analysis of pottery:

1. Decorative variation responds to a quite different set of specific cultural factors than mechanical variation. Ceramic typologies and seriation models that combine both decorative and mechanical criteria, without prior consideration of their different cultural meanings, are at best inappropriate for studies of cultural process (e.g., Braun 1980b).

2. The mechanical uses of ceramic vessels directly constrain the kinds of decoration they receive and hence the kinds of social information they carry. The constraint here goes beyond simply the limiting of decoration to those vessel parts that are visible and not likely to become obscured by residues or abrasions. Rather, the constraint arises from the fact that the kinds of social information conveyable by an artifact, and hence the kinds of decoration they are likely to receive, vary with the artifact's size and its context in the social environment (Wobst 1977). As a result, different functional classes of vessels can receive different kinds of decoration (Plog 1980:17–19, 85–98). Further, different functional parts of the same vessel can be treated as distinct decorative fields, each receiving a different kind of decoration (Friedrich 1970; Plog 1980:17–19). In addition, even within a single functional class of vessels, vessels of different sizes may receive different decorative treatments according to their possibly differing contexts of use.

Variation in the conditions of use or breakage occurring within different size and functional classes could affect the appearance of decorative variation in the archaeological record, even in the absence of any variation in the organization of decorative behavior. Most deposits of pottery result from vessel failure through use (e.g., David 1972; DeBoer 1974; DeBoer and Lathrap 1979; Foster 1960). It follows, then, that variation over time or space in the pattern of use of different functional classes or sizes will result in variation in the frequencies or contexts in which they are broken and discarded. Under such circumstances of variation, collections of pottery excavated from different contexts of deposition may not be completely comparable. If the collections contain samples of different parts of the

functional assemblage, they may also contain samples of different parts of the decorative system or assemblage. Ideally, then, the researcher engaged in an analysis of ceramic decorative behavior must control for the organization of ceramic vessel use as well.

Thus, both in reasons and the reasoning necessary for analyzing pottery in terms of the organization of its use already exist. We lack neither the appropriate theory nor the methods, but only their integration.

## APPLICATIONS

The mechanical performance characteristics of ceramic vessels can be analyzed archaeologically in three different ways. First, we can analyze those physical properties of vessels that the potters could have controlled to achieve particular performance characteristics. These properties often are routinely recorded in ceramic descriptive studies, albeit for very different purposes. These properties include variables of the whole-vessel profile, the cross-sectional shape of the wall at different points along the profile, and the wall's material composition. The last of these sets of variables includes variables of chemical composition (e.g., Rye 1976, 1981:29–57), but also includes mechanically more important characteristics of temper particle and pore size, shape, density, and arrangement (e.g., Braun 1982).

Second, we can directly measure performance characteristics in the laboratory. These characteristics could include various aspects of breakage strength and elasticity, various aspects of resistance to thermal shock and fatigue, and thermal conductivity. Procedures are available for measuring a wide range of such engineering characteristics in modern ceramics (e.g., Hench and Gould 1971; Jones and Berarb 1972; Kingery *et al.* 1976). These procedures appear to work equally well with archaeological materials (e.g., Shepard 1968:125–136; Steponaitis 1980).

A major consideration in the archaeological use of such procedures, however, is that under almost all circumstances we will be measuring what may be termed remnant or *residual* performance. The overwhelming majority of archaeological ceramic deposits results from vessel failure through use. Further, whole vessels or fragments, deposited either as *de facto* refuse (*sensu* Schiffer 1976) or as ritual paraphernalia, as well as fragments of failed vessels, will all have been subject to postdepositional changes. Consequently, we can measure only the ability to perform that remains after failure and/or post-depositional deterioration. Without evidence of whole-vessel and wall-sectional design (e.g., shape and composition, as mentioned previously), direct measurements of thermal shock resistance, for example, cannot distinguish between a cooking vessel that failed through use and a

vessel that was never intended for thermal shocking in the first place. Step-onaitis (1980) presents an example of the use of complementary data on both design and performance to evaluate the organization of one society's vessel assemblage.

The third approach to the study of the mechanics of vessel use involves the analysis of the physical effects of use. These effects include fracture, spalling, and breakage patterns; residues and preserved contents; and wear patterns. Patterns of fracture and breakage previously have found use as classificatory criteria in culture-historical studies (e.g., Shepard 1968:137). However, the arrangement and orientation of planes of fracture also indicate the directions of stresses applied to the original vessel, and the means used to provide resistance to fracture from those stresses (e.g., Davidge 1979). Moreover, information on relative frequencies of breakage and contexts of deposition of different vessel forms can provide additional means for analyzing whole-vessel use (e.g., DeBoer 1974).

### Analysis of Vessel Use: A Woodland Example

A full exposition of the engineering principles involved in the mechanical performance of preindustrial pottery, as presently understood, is not within the scope of this chapter. However, the central premise here—that measurements of ceramic technical variation routinely used for culture-historical purposes can also inform us about variation in whole-vessel use—is empirically demonstrable.

The examples below are drawn from studies of Woodland period pottery (ca. 600 B.C.—A.D. 900) from western Illinois and eastern Missouri. This pottery presents a useful case in point for several reasons: First, in a society with a mixed subsistence base of hunting, fishing, collecting, and horticulture, and without any permanent hierarchical differentiation among settlements, pots may be presumed to have been domestic products. Second, the ceramic vessel assemblage was relatively simple, consisting almost exclusively of a jar form with a proportionately wide mouth. Third, the pottery has received considerable culture-historical attention for nearly a century (e.g., since Homes 1903:186–194), as well as attention more recently from the standpoint of vessel function (Braun 1977, 1982).

The wide, unrestricted orifice relative to vessel width and height identifies the Woodland jar form as a generalized mixing and/or cooking container (e.g., Braun 1980a, 1980b; Smith 1981). Variation in the physical characteristics of Woodland pottery therefore must be viewed in the context of Woodland period diets. Analyses of faunal and floral indicators (Asch *et al.* 1979; Ford 1974, 1977; Johnson 1979; Kay 1979; Kuttruff 1974; Mun-

son *et al.* 1971; Parmalee *et al.* 1972; Struever 1968; Styles 1981) reveal a continuum of complexly scheduled, broad-spectrum subsistence practices throughout the central midwestern Woodland period. Subsistence mainstays were white-tailed deer and several varieties of riverine fish, and hickory nuts and acorns.

The dietary importance of seed foods and cultivation appears to have increased throughout the region's Woodland continuum. Prior to circa A.D. 700 maize constituted a trivial supplement to a cultivated complex of squash, gourd, and several native annuals producing either relatively oily seeds (two species) or relatively starchy seeds (three species) (Asch and Asch 1977; Asch and Asch 1978; Asch *et al.* 1979; Bender *et al.* 1981; Cowan 1978; Ford 1974, 1977, 1979; Kuttruff 1974; Munson *et al.* 1971; Struever 1968; Struever and Vickery 1973; Yarnell 1978). This complex first appeared during the preceding Late Archaic period (ca. 3000–600 B.C.), but its use appears to have accelerated after circa 200 B.C. The relative use of cultivated foods increased further after circa A.D. 400–450, primarily in the use of the more starchy native seeds; after circa A.D. 700 maize (a highly starchy seed food) increasingly came to dominate the assemblage of cultigens. (These dates are regional approximations. Rates of change presumably varied somewhat from one locality to the next, according to local conditions.)

As Hargrave and I have noted elsewhere (Hargrave 1981; Hargrave and Braun 1981), the starchy seeds consumed during the Woodland period were small and moderately hard textured (e.g., Asch and Asch 1977; Asch and Asch 1978; Cowan 1978). Raw consumption would require pulverization or very intense chewing, but would not provide the user with the seeds' full potential food value. Examinations of Late Archaic–Early Woodland coprolites from eastern Kentucky suggest that seeds were consumed raw or only slightly roasted, and so were not consumed in a fully digestible state (Asch and Asch 1978:302–303; Cowan 1978:269; Yarnell 1977). Given the wide range of cultural similarities between eastern Kentucky and western Illinois at the start of the Woodland period, it is not unlikely that similar practices of seed food preparation were followed in both areas during that time.

Both the palatability and digestibility of starchy seeds can be enhanced by cooking them to the point of gelatinization in a liquid broth. Gelatinization of starches, however, requires longer cooking times and higher temperatures than the cooking of any other foodstuff. Furthermore,

The rate of gelatinization increases with cooking temperature, making boiling the most efficient form of cooking for starches in a liquid mass [Peckham 1974:208]. The temperature of the external heat source does not affect the

boiling temperature of a cooking liquid (approximately 100 degrees Centigrade), but does affect the rate at which the liquid is brought to a boil and the consistency with which the boiling is maintained. Consequently, we may expect that an increasing importance of boiling starchy broths would . . . involve increasing levels of heat intensity and greater rates of temperature change in the use of cooking jars [Hargrave and Braun 1981:12].

The frequency of juvenile dental caries increased markedly between circa A.D. 0–200 and A.D. 600–800 in west-central Illinois. This suggests that seeds were routinely being boiled to a point of gelatinization by late in the Woodland period (Buikstra 1977:78; Cook 1979; Cook and Buikstra 1979). Unfortunately, comparable skeletal data showing the timing and gradation of change are not yet available for the period between circa A.D. 200 and 600.

Against this backdrop, the following changes in ceramic technology are recognized in the culture-historical literature:

1. The earliest ceramic vessels (ca. 600–200 B.C.) were relatively squat and cylindrical, with a flat or conoidal base, usually thick walls (10–15 mm), and very coarse temper inclusions of crushed quartz-rich rock. This so-called Early Woodland pottery gave way after circa 200 B.C. to taller, relatively more elongate, cylindrical vessels, with a blunt or conoidal base, a somewhat thinner range of wall thicknesses (6–12 mm), and somewhat finer temper inclusions of sand and crushed rock.

2. By circa A.D. 1 the smallest vessel sizes begin to occur occasionally as burial offerings, and represent some of the finer examples of decoration in the assemblage. By circa A.D. 100 this range of smaller, finer vessels is joined—and in some areas possibly replaced—by a similarly shaped range of smaller vessels with a distinct repertoire of even more finely rendered decorations and very thin walls, and with crushed limestone for tempering among the finer specimens. These latter vessels conventionally are classified as a separate “ware,” so-called Hopewell Ware, because of their apparently distinctive characteristics of decoration and, sometimes, temper. Hopewell Ware rarely comprises more than 1% of the sherds in domestic refuse, but most of the vessels occurring with contemporary burials are of this type (e.g., Braun 1979). Vessels of Hopewell Ware, and a range of morphologically and decoratively similar vessels produced contemporaneously throughout the eastern United States, also appear to have served as media of supralocal exchange (e.g., Struever and Houart 1972; Toth 1979). After circa A.D. 200, Hopewell pottery ceases to be identifiable as a separate part of the ceramic assemblage. Most of its formerly distinctive technical characteristics become common to the entire domestic assemblage, whereas its decorative characteristics disappear altogether.

3. Between circa A.D. 200 and A.D. 800–900, in general, the central Midwest sees a shift in the domestic jar form from the preceding elongate vessels toward more squat vessels, globular below the neck or shoulder; with the hemispherical base, thin walls (3–5 mm by A.D. 800–900), and fine sand and crushed rock temper inclusions. After circa A.D. 900–950, the end of the Woodland period, the ceramic assemblage undergoes a radical transformation. The assemblage becomes morphologically differentiated and most forms receive crushed shell tempering. These changes are part of a reorganization of the entire sociocultural system, and will not be addressed in this paper. (Major descriptive works on the regional Woodland pottery are Chapman 1952, 1980; Griffin 1952; and Struever 1968; see also Benn 1978; Cole and Deuel 1937; Fowler 1952, 1955; Griffin 1965; Griffin, *et al* 1970; Kay and Johnson 1977; McGregor 1958; Maxwell 1951; O'Brien 1972; Ozker 1977; Stephens 1975; Struever 1964, 1965; Vogel 1975).

The above changes in Woodland ceramic technology fall into three areas: change in wall sectional shape; change in tempering characteristics; and change in whole-vessel shape. All three indicate the same pattern of change in ceramic vessel use, as seen through different aspects of mechanical design.

**Wall-Sectional Shape** The thickness of a ceramic vessel's wall affects three aspects of mechanical performance: thermal conductivity, flexural strength (breakage load), and resistance to thermal shock. The thermal conductivity of a vessel wall is its ability to conduct heat from one face to the other, determining the rate at which a change in temperature at one face will be transmitted to the other. Other things being equal, the thinner a wall, the higher its thermal conductivity (Van Vlack 1964:117–165).

The flexural strength of a vessel wall is its ability to accept mechanical stresses without distorting or, ultimately, fracturing. Mechanical stresses can arise from the movement, stacking, or accidental dislodging of a vessel, or from striking or compressing it with another object. Resistance to distortion or fracture depends on both the number and strength of various bonds within the wall's crystalline matrix. Flexural strength therefore tends to decrease with decreasing wall thickness (e.g., Jones and Berarb 1972: 147–148; Rado 1969:194, 199). This latter effect, however, depends on the degree of wall curvature. The smaller the radius of curvature of a wall of any given thickness, the higher will be its resistance to mechanically induced fracture.

Thermal shock resistance, finally, is a vessel's ability to withstand the stresses generated by a sudden and extreme change in its thermal environment (e.g., Rye 1981:26–27). Examples of thermal shocks include the placing of a cool vessel over a cooking flame, or the removal of a hot vessel from

a fire or cooking oven (e.g., Steponaitis 1980). Thermal shock resistance increases with decreasing wall thickness, being in part a function of the wall's thermal conductivity (e.g. Lawrence 1972:174–183; Rado 1969: 198–199). All three of the preceding aspects of mechanical performance will of course also depend on wall composition (see following material).

Viewed by itself, then, the change in wall thicknesses along the Woodland ceramic sequence suggests a single mechanical trend. The importance of thermal conductivity and thermal shock resistance appears to have increased throughout the sequence relative to the importance of flexural strength. The change in wall thickness need not be taken to represent a decrease in the absolute importance of flexural strength, however. The change simply indicates a shift in the balancing of flexural vs. thermal concerns, as these affect one particular aspect of vessel construction. Viewed against the archaeobotanical record, the trend in wall thicknesses suggests an increasing attention to the extraction of digestible nutrition from starchy seed foods through cooking—presumably through simmering or boiling rather than parching or popping (e.g., Braun 1980b; Ozker 1977).

The change in wall thicknesses over time, however, is not a simple linear trend. Figure 5.1 summarizes measurements of wall thickness on sherds associated with radiocarbon-dated charcoal from 15 pit features, from habitation sites in the lower Illinois River valley and an immediately adjacent section of the central Mississippi River valley. (These and the measurements discussed later in this paper were obtained as part of research

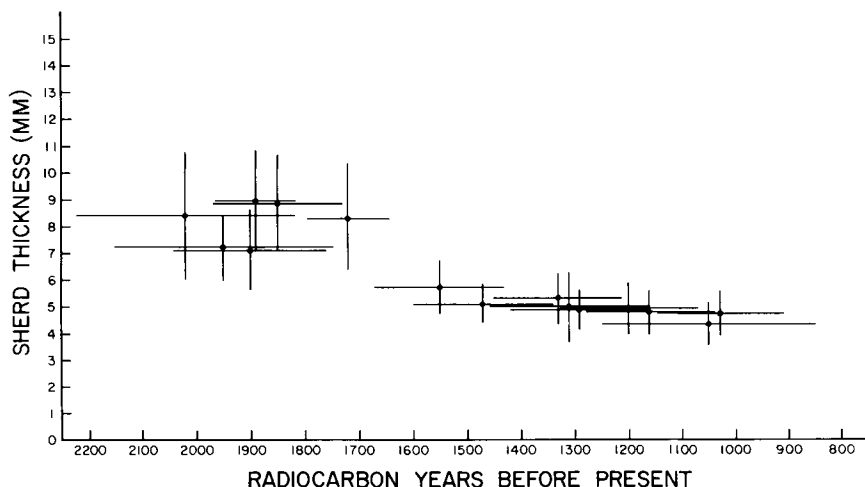


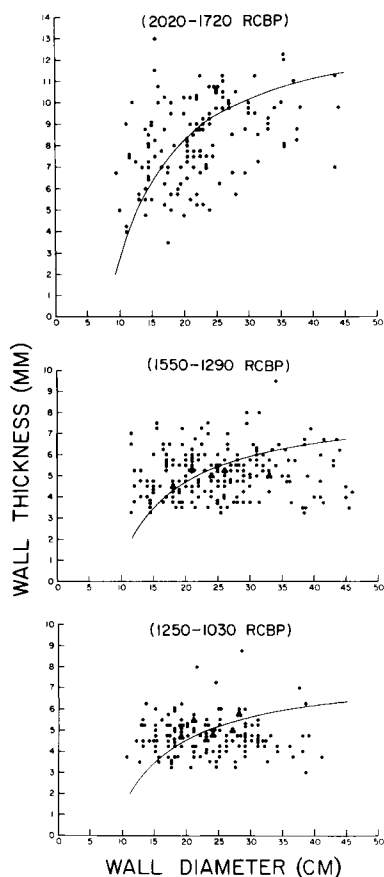
FIGURE 5.1. Sherd thickness versus radiocarbon age of associated charcoal, for 15 ceramic samples from west-central Illinois. Each sample is represented by the sample mean and a one-standard-deviation bar for both thickness and age. Base sherds are excluded.

in progress on Woodland ceramic chronometry [e.g., Braun 1982; Hargrave and Braun 1981]) Features lacking acceptable cultural depositional integrity or sample sizes of at least 10 measurable sherds are excluded from consideration. Features dating prior to 2020 B.P. (70 B.C., uncalibrated), and between 1720 and 1550 B.P. (A.D. 230 and 400, uncalibrated), have not yet been available for analysis. As Figure 5.1 indicates, nevertheless, the samples dating prior to circa 1550 B.P. exhibit not only higher mean thicknesses, but wider ranges of variation both within and among samples.

The reason for the variability in thickness measurements among the pre-1550 B.P. sherds can be seen in Figure 5.2. The earlier material exhibits a broad trend of increasing thickness with increasing diameter of curvature; the later material does not. Curvature here was measured with the gauge held perpendicular to each sherd's approximate center, along the axis of minimum curvature. The axis of minimum diameter in most cases represented the horizontal axis of the original vessel profile. Each curvature thus measured represents vessel girth at the sherd's place of origin on the vessel profile, and so is affected by both vessel size and shape.

Figure 5.2 illustrates several points. First, because wall thickness in the earlier pottery varies in part with wall curvature, the mean thickness of each sherd and each small deposit of broken pottery represents a sample from a potentially different part of an engineering spectrum. This spectrum also indicates a concern for the flexural strength of vessel walls. Second, the contrast between the pre- and post-1550 B.P. material indicates either a reduction in the potters' concern for flexural strength, or the development of means for producing thin walls without sacrificing flexural strength at any diameter of curvature. As is indicated below in the discussion of temper, the latter interpretation appears far more plausible. Third, there appears to have been a marked change in the balancing of concerns for flexural versus thermal stresses between circa 1720 and 1550 B.P. Such a change is consistent with interpretations of an increased importance of the boiling of starchy seed foods after circa 1550 B.P. (e.g., Asch *et al.* 1979), further suggesting that this particular increase occurred relatively rapidly (in an archaeological sense).

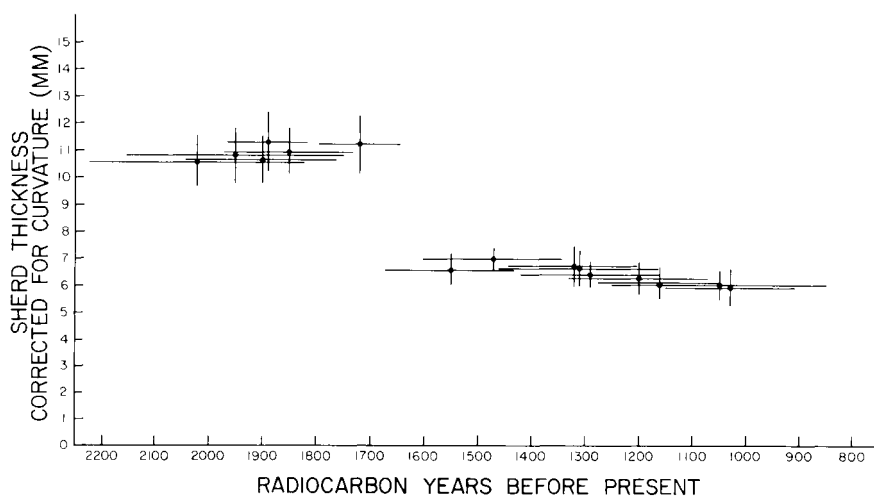
Figure 5.2 also illustrates the reduced major axes (first principal components) between wall thickness and the inverse exterior diameter. These axes can be used to control or correct the measurements of wall thickness for the compounding or interactive effects of wall curvature, by correcting each thickness value relative to an arbitrary reference diameter of wall curvature. A reference diameter of 40 cm was used in this instance. When the measurements are corrected, the range of variation in thicknesses both within and among temporally adjacent samples drops sharply (Figure 5.3). The standard deviations of thickness among the pre-1550 B.P. samples aver-



**FIGURE 5.2.** Wall thickness versus Wall diameter for three consecutively dated sets of samples from west-central Illinois. The samples are the same as in Figure 5.1. The superimposed lines are the reduced major axes of correlation between thickness and inverse exterior diameter: (a) 2020–1720 B.P.,  $y = (-112.5 \div x) + 13.86$ ; (b) 1550–1290 B.P.,  $y = (-73.68 \div x) + 8.35$ ; (c) 1250–1030 B.P.,  $y = (-63.03 \div x) + 7.69$ . Base sherds are excluded.

age 1.8305 mm for the uncorrected measurements, and 0.9941 mm for the corrected measurements. Among the post-1550 B.P. samples the averages are 0.9244 mm and 0.6058 mm, respectively.

The results of correcting wall thickness relative to wall curvature (Figure 5.3) emphasize the contrast between the earlier and later samples and again support an interpretation of a rapid change in the balancing of concerns for flexural versus thermal stresses between circa 1720 and 1550 B.P. The relative abruptness of the shift after circa 1720 B.P., and the slower but continuing reduction in corrected wall thicknesses after 1550 B.P., also indi-



**FIGURE 5.3.** Sherd thickness, corrected for a 40-cm reference wall diameter, versus radiocarbon age. The samples are the same as in Figures 5.1 and 5.2. Each sample is represented by the sample mean and a one-standard-deviation bar for both corrected thickness and age. Base sherds are excluded.

cate that this measurement has considerable potential as a ceramic chronometric device within that time span.

One final point needs mention concerning the relationship between wall thickness and curvature. As noted earlier, the descriptive literature on midwestern Woodland ceramics dating between circa 200 B.C. and A.D. 200 conventionally distinguishes between a thicker domestic ware and a thinner, specialized ware. Figure 5.2 (a) shows that both supposed “wares” in fact are part of a single engineering spectrum, with wall thickness varying in part as a function of vessel size. If the decoration of the thinner—smaller vessels and their use in exchange is as distinct as the literature suggests, then the case is consistent with the idea (already postulated) that decorative treatment will vary with vessel size and the contexts of a vessel’s visibility, even within a single functional class (e.g., Wobst 1977). There were not sufficient numbers of sherds bearing decoration in the samples considered here, however, to permit a more detailed consideration of this subject.

**Temper** The archaeological and ethnographic literature generally explains the use of temper as a means for controlling paste plasticity during vessel manufacture; for binding the paste during vessel drying prior to firing, to reduce the stresses resulting from paste shrinkage; and for increasing the flexural (rupture) strength of the dried vessel walls prior to firing (e.g., Arnold 1971; Lawrence 1972; Rado 1969:4, 23; Rye 1976; Rye and Evans 1976; Shepard 1968:25–27; van der Leeuw 1976:84–85). These

effects result from a more specific effect of tempering on a ceramic body's resistance to cracking.

Resistance to cracking can be divided into two parts. *Resistance to crack initiation* involves the ability to prevent cracks from forming, and depends on the clay molecules being able to form a continuous, homogeneous crystalline matrix. *Resistance to crack propagation* involves the ability to prevent cracks from enlarging and spreading once formed, and depends on the presence of irregularities in the matrix. These irregularities act as points of focus for internal stresses, and therefore attract microfractures that develop to relieve these stresses. The microfractures, in turn, are truncated by the crystalline irregularity. Each episode of stress entails another cycle of crack formation and truncation. Resistance to crack propagation increases a vessel's use life.

The chemical bonds among clay particles in an unfired paste are weak relative to the bonds formed during firing. Resistance to crack initiation consequently is low. The need for tempering to increase resistance to crack propagation correspondingly is high. Shrinkage-related stresses will also be greater, the larger the vessel and/or the thicker its walls; and hence the greater will be the need for a binding temper (e.g., Lawrence 1972:93–101).

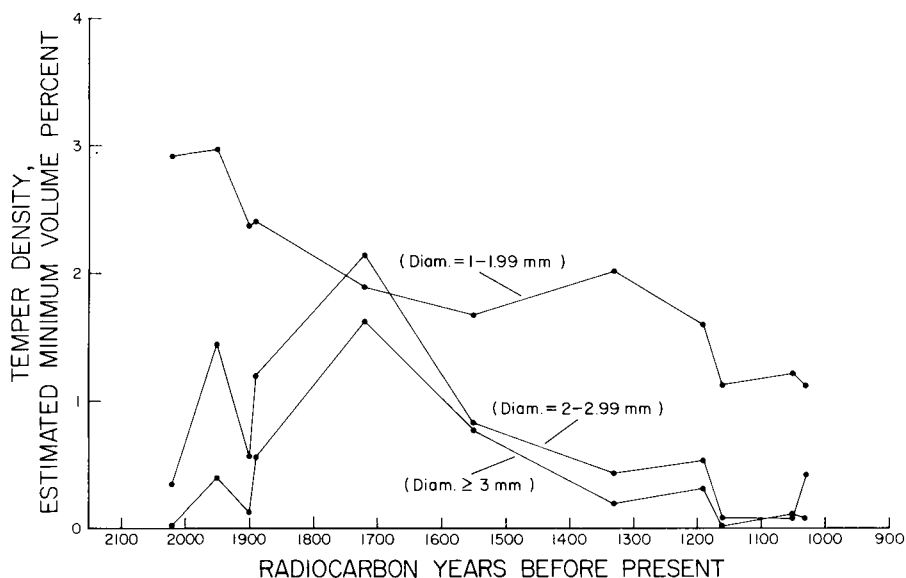
Resistance to crack initiation in fired pastes increases with decreasing grain size in the fired paste (Chu 1968; Davidge 1979:64–117; Kingery *et al.* 1976:768–813; Kirchner 1979:1–12; Rado 1969:194; Shepard 1968:26–27, 131). Cracks can be initiated by mechanically or thermally induced stresses. Thermal stresses can arise either by thermal shocking and the production of a pronounced expansion or contraction differential between a wall interior and exterior, or through the differential thermal expansion or contraction of temper inclusions relative to the fired clay (e.g., Hasselman 1969; Lawrence 1972:174–182; Rado 1969:30, 191–200; Rye 1976:113–120; Shepard 1968:24–31, 131; Steponaitis 1980:28–83). The smaller the average grain size of tempering inclusions, then, the higher the flexural strength of a vessel's walls, and the higher its resistance to crack initiation under thermal stress.

Resistance to crack propagation in fired pastes, on the other hand, increases (apparently within limits) with increasing grain size of irregularities included in the paste (Kingery *et al.* 1976:796; Lawrence 1972:181–182; Rado 1969:191; Steponaitis 1980:28–83). The shape, density, and orientation of the irregularities—in our case, temper inclusions—also appear to affect this latter relationship (e.g., Lawrence 1972:102–110, 121–126; Shepard 1968:24–31, 125–136; Steponaitis 1980:28–83). The ability of temper inclusions to increase resistance to crack propagation in fired pastes, of course, depends on the similarity in thermal expansion characteristics between the temper and the paste. The greater the temperatures

of use of a vessel and/or the larger the temper particles, the greater will be the stresses induced by any differential thermal behavior of the temper (e.g., Rye 1976:116–118).

Figure 5.4 summarizes measurements of the size distribution of temper particles among the Woodland ceramic samples from the lower Illinois River valley discussed earlier. The measurements were obtained with an experimental radiographic technique for estimating the minimum volumes occupied by particles of different sizes in a fired paste, expressed as a percent of the volume of the paste (Braun 1982). The tempering materials are particles of either crushed rock, consisting primarily of quartz crystals with occasional admixtures of feldspar, hornblende, and limestone; or sand, also consisting primarily of quartz (Fowler 1955; Griffin 1952; McGregor 1958:210–211; Struever 1968:140–173). Quartz differs significantly from clay minerals in its characteristics of thermal expansion, even at relatively low temperatures of firing and use (Rye 1976:116–117).

The overall trend in tempering characteristics over time consists of a reduction in the density of all particle sizes greater than 1 mm in average diameter, and a decrease in average particle size overall. These tendencies suggest both an increasing use of temper to manipulate flexural strength, and an increasing concern for stresses resulting from differential thermal



**FIGURE 5.4.** Temper density, estimated minimum volume percent, versus radiocarbon age, for 11 ceramic samples from west-central Illinois, after Braun (1982). The samples are the same as in Figures 5.1–5.3, but without 4 samples with fewer than 10 analyzable radiographs. Base sherds are excluded.

behavior of the temper. Viewed alongside the changes in wall sectional shape, then, the changes in tempering characteristics also indicate an engineering trend of increasing attention to the accommodation of use-related thermal stresses.

**Whole-Vessel Shape** Sufficient metric data do not yet exist to permit a quantitative evaluation of changes in Woodland whole-vessel shape. A couple of comments may be made, however, based on the reported changes in particular vessel parts. Points of abrupt angular change in the wall profile of ceramic vessels tend to become points of concentration of mechanical and thermal stresses (e.g., Amberg and Hartsook 1946; Rye 1976:114). The decline in the use of a flat basal shape with its associated sharp corner angle and the overall shift from cylindrical to, eventually, globular vessel shapes both suggest an overall Woodland trend of (or response to) increasingly stressful conditions of vessel use. The reasons for the initial emphasis on cylindrical vessel shapes and nonhemispherical bases, however, remain to be investigated (cf. Braun 1980b; Linton 1944; Munson 1976; Ozker 1977; Smith 1981).

In summary, many of the characteristics of Woodland pottery used in culture-historical systematics appear to have varied for technical reasons related to the mechanical use of the pottery. The changes in wall-sectional shape, temper, and whole-vessel shape provide complementary indications of the use of the vessels under increasingly thermally stressful conditions. These conditions demanded higher thermal conductivity and higher resistance to thermal shock, probably under higher temperatures of use. The variation in technical characteristics thus arises from changes in subsistence and dietary practices. When examined quantitatively in relation to independent temporal controls, consequently, the technical variation not only informs us about the existence of change in subsistence and dietary practices, but also provides information on the *rate* of change not readily available in the archaeobotanical and zoological records. Knowing what the technical variation means, we now can more fully exploit its potential for both chronometric and behavioral inferential purposes.

## CONCLUSIONS

Archaeology possesses an impressive array of techniques for studying the nondecorative characteristics of pottery for purposes of historical description. Despite the opportunity, however, we have barely attempted to explain the observed variation (beyond placing it in its geological context). The need for explanation becomes clear when we recognize that characteris-

tics of vessel morphology, composition, manufacture and disposal, as well as decoration, vary according to different sets of cultural and engineering constraints. If we gave greater theoretical consideration to those constraints before measuring our sherds, we could vastly improve our ability not only to use pottery for culture-historical reconstructions but also to explore a variety of aspects of prehistoric behavior.

When we do seek to explain ceramic technical variation, it becomes clear that much of it results from variation in the mechanical demands placed on the vessels during manufacture and, more importantly, during use. This paper has reviewed the engineering concept of mechanical performance characteristics, at a very general level, as this concept applies to pottery. It was noted that potters can control the mechanical performance characteristics of pottery by manipulating vessel size and shape, and the composition and cross-sectional shape of the vessel walls. The potters thereby can control not only the suitability of a vessel's shape for a particular range of tasks, but also the vessel's mechanical efficiency and its ability to perform over a reasonable use life without failure. Culture-historical typologies that combine attributes of vessel size and shape, and the composition and cross-sectional shape of the vessel walls, therefore often will be recording functional variation, although not necessarily in a readily interpretable way.

Finally, as the Woodland example illustrates, variation in the demands on mechanical performance can simultaneously affect numerous ceramic characteristics even among relatively simple assemblages. Consequently, we can evaluate our explanations by examining and comparing several complementary classes of evidence, occasionally giving a refreshing power to our evaluations.

## ACKNOWLEDGMENTS

Michael L. Hargrave and M. Denise Hutto provided invaluable assistance in the research on the Illinois Woodland pottery. The Northwestern University Archaeological Program, the Center for American Archaeology, and the University of Michigan Museum of Anthropology provided the collections and provenience data for the Illinois valley research, through the assistance of James B. Griffin, David Morgan, William Parry, Frank Rackerby, Thomas Rocek, John Speth, Stuart Struever, and Michael Wiant. Material support has come from Southern Illinois University, from the Office of Research Development and Administration, the Department of Anthropology, and the Center for Archaeological Investigations, and from the Northwestern University 1980 Summer Field School. Tom Gatlin prepared the illustrations. Vin Steponaitis and Marion Smith have freely shared their own work and ideas, and I am most grateful to them. I hope the chapter does justice to their work.

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## **PART III**

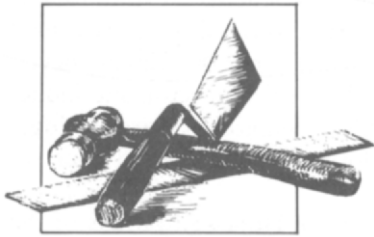
# **THE PRECEDENTS OF THE ARCHAEOLOGICAL RECORD**

The products and precedents of the archaeological record are not simple empirical entities. The products—the motherlode of archaeological mining—are demonstrably present. The littered tabletops in labs across the country make any metaphysical discussion of their reality superfluous. What adds complexity to the issue is the partial and fragmentary context in which archaeological products reside. The context—what we call the precedents of the archaeological record—does not reside in artifacts, in associations, or in Euclidian space. It is behavioral and social; it is, for the archaeologist, the product of conceptual labor. The archaeological record is then the interaction of object and concept. The products—the objects—offer us the opportunity to produce an infinity of measurements; the concepts identify which objects and which attributes have behavioral and social meaning. It has often been pointed out that the archaeological record is static. In one sense this is true: the objects themselves are not going anywhere in a hurry. Yet in another sense, the archaeological record is an exemplar of dynamism: a new concept creates a new archaeological record with new attributes, new associations, and new contexts.

Turning to an examination of the conceptual context of archaeological research, we find that there is a clear relationship among the concepts brought to our research, the character of the archaeological record we “discover,” and the questions we believe that the archaeological record can be made to address. It is important for us to recognize that the archaeological record is not impervious to outside influence. To a limited extent, the concepts we bring to the products of the archaeological record create that record. For any question we ask, the richness or the poverty of the archaeological record depends on both products and precedents. An abundance of artifacts in isolation will not answer our question, nor will concepts by themselves, no matter how elegant, reveal to us the laws of social motion.

Defining our questions carefully and conscientiously does not in itself mean that we will necessarily be able to answer those questions. We need to resolve the contradictions that often exist between our questions and the concepts we attempt to apply to those questions. It is this area of conceptual labor that the papers in this section address. Familiar concepts of behavior and social change—settlement, subsistence, population growth, stratification—are reviewed; the conceptual blindspots are identified; and the limits that these blindspots impose on the archaeological record are discussed. But these chapters go beyond the mere identification of conceptual hammers and lead toward the resolution of conceptual contradictions. This is no small project for it entails the recasting of the archaeological record to a form demanded by our questions. New attributes and associations are identified, and new measurements are suggested. Given the scale of the task, it is understandable that some of the authors are more successful than others in inventorying the archaeological record that they have invented, but in all cases the general form of their new archaeological reality is apparent.

It would be going much too far to claim that there is a strong conceptual unity among the chapters—it is clearly not the case. The questions are different; the concepts developed are different. In this volume, the reader will find neither methodological nor conceptual needle’s eye through which the products of the archaeological record must pass on our route to understanding. What unites the chapters is their concern for the development of concepts that address social relations, not individualizing behaviors.



# 6

## Biology, Behavior, and Borrowing: A Critical Examination of Optimal Foraging Theory in Archaeology

ARTHUR S. KEENE

*Use an idea three times and it's yours.*

George J. Armelagos

### BACKGROUND

In 1968 Lewis Binford challenged archaeological pessimists with his assertion that our knowledge of the past is limited largely by our methodological naïveté, and that we can understand the past in considerable detail if we only develop the appropriate methods (Binford 1968:22–23). Few outside the discipline of archaeology, and probably few within the discipline, shared Binford's optimism; but the proliferation of archaeologi-

cal method in the subsequent 15 years can be viewed as an attempt to convince ourselves that Binford's words are true. During that productive period, American archaeology witnessed an explosion of new methods, theories, models, and ideas, as archaeologists confronted questions that were once considered to be outside their purview (e.g., social boundaries, information processing, and demography). The sources of innovation and inspiration for this generation of archaeologists frequently lay outside the discipline of anthropology.

Archaeologists borrowed and consumed ideas, methods, and theories from a variety of other fields. Economics, geography, and population ecology were the most frequent donors. Certainly these disciplines have contributed significantly to our ability to understand the past; however, sometimes our excursions into other fields have resulted in literal adoption rather than *adaptation* of models and methods to our own specific needs. In our fervor to upgrade our analyses and employ the latest methods, we sometimes adopt new techniques without regard for their original intent or limitations. This, of course, is a problem inherent to interdisciplinary borrowing in general and is not unique to archaeologists. Borrowing can hold great promise for archaeology as a stimulus to both analytical rigor and creative thought; but uncritical borrowing and rote application of methods developed in other fields may pose numerous hazards. In this chapter I will examine some of the pitfalls that can accompany careless borrowing. The issue is not a new one in anthropology and has been discussed at length by others (Curry 1980; Morales and Levins 1974; Service 1969; Spaulding 1977; Thomas 1978; Thomas *et al.* 1979). I will limit my comments to the dependence of archaeology on biology for models and theories of change and will offer a few caveats.

It is not my intent to argue that borrowing from other fields is inappropriate. Rather, I would like to focus on the way we use these exogenous innovations and the way they may come to limit our field of vision. A number of the pitfalls I will discuss are inherent, but not exclusive, to archaeological applications of method and theory developed within the field of evolutionary ecology (Pianka 1978), particularly optimal foraging theory (Krebs 1978; Pyke *et al.* 1977). I have chosen to use optimal foraging theory as an example because it is still a relatively new addition to the archaeologist's tool kit. The rush to jump on the optimal foraging bandwagon in the past few years has been impressive (e.g., Beckerman 1980; Bleed and Bleed 1981; Dincauze 1980; Keene 1979, 1981; Lewis 1979; Perlman 1976; Redding 1981; Yesner 1981; and others); and if other methods discussed in this volume represent archaeological hammers, optimal foraging theory could well become the archaeological pile driver of the 1980s. Because

optimal foraging theory is such a new and elegant innovation, it is useful to investigate its limitations now.

## A BRIEF INTRODUCTION TO OPTIMAL FORAGING THEORY

Archaeologists have long noted parallels between animal behavior and human behavior. This has recently led some to suggest that our understanding of human foraging strategies may be enhanced by empirical and theoretical research on the behavior of nonhuman foragers. The work of Charnov (1976), MacArthur and Pianka (1966), and Wiens (1976), to name a few, raises questions about the nature of foraging among nonhumans that are also relevant to the study of human foragers. For example, in studying human foragers we have been especially concerned with factors that govern food selection and scheduling of resource utilization. Why are some resources favored over others? Given certain requirements and constraints, which resources can best be exploited at what locations, at what times of the year, and in what quantities? How do predators avoid overexploiting a prey population? What are some of the advantages of an aggregated versus a dispersed settlement pattern? What territory size can be exploited with maximum security and efficiency? What are the costs and benefits to be derived from the establishment and defense of a home territory? These microecological questions are not only fundamental to the study of biocultural adaptations, but they form the foundation of much past work in the study of prehistoric subsistence and settlement systems (e.g., Ford 1977; Jochim 1976; Osborn 1977; Thomas 1973; Vita-Finzi and Higgs 1970; Wood 1978). At the very least, these questions allow us to isolate biological stresses on a population and examine the efficacy of varied responses to these stresses. By placing aspects of these questions within an explicit cost–benefit framework, evolutionary ecologists have been able to develop clusters of simple models that can produce hypotheses about expected foraging patterns in different environments (see Winterhalder and Smith 1981). These models are commonly referred to as optimal foraging models.

Various optimal foraging models have attempted to generate counterintuitive hypotheses about the “best” strategies for particular circumstances in three primary areas:

1. Food choice and dietary composition (optimal diet)
2. Group organization (optimal group size)
3. Site location and patch use (optimal foraging space)

Optimal foraging models are essentially based on the following neo-Darwinian assumptions:

1. Natural selection and competition are the inevitable outgrowth of reproduction in a finite environment.
2. Adaptive processes select for behaviors that allow an organism to efficiently and effectively achieve life goals. Greater efficiency in achieving these goals confers greater fitness (Pianka 1978; Thomas *et al.* 1979:10).

Within the bounds of these assumptions, optimal foraging models examine the costs and benefits associated with different activities and predict an optimal strategy. Assessing behavior within an optimization framework requires a currency, or cost–benefit measure that is significant with regard to adaptive goals. The currency employed almost exclusively in optimal foraging studies is energy (cf. Keene 1981), for as Lotka notes

In the struggle for existence, the advantage must go to those organisms whose energy-capturing devices are most efficient in directing available energy into channels favorable to the preservation of the species [1922:147].

Thus, it is argued that selective advantage is conferred upon those individuals that maximize their efficiency of energy capture. In doing so, the individual not only satisfies its basic metabolic needs, but frees time that can then be devoted to other activities that enhance fitness, such as the search for mates and avoidance of predators (see also Kirch 1980; Smith 1979; Thomas *et al.* 1979 for further discussion).

Work in evolutionary ecology has implicitly if not explicitly influenced much of the recent archaeological research concerned with prehistoric diet, demography, and spatial organization (e.g., Bayham 1977; Jochim 1976; Keene 1979; Reidhead 1976; Wilmsen 1973). Optimal foraging applications have been undertaken with a variety of objectives in mind. For example, Perlman (1976) and Yesner (1981) use the archaeological record as a means of evaluating the validity of optimization principles. Others have used optimal foraging theory as a formula for prospecting, that is, generating expectations about the locations of archaeological sites (e.g., Institute for Conservation Archaeology 1979). However, the most common archaeological applications of optimal foraging theory have been attempts to generate alternative interpretations of a body of archaeological materials (e.g., Keene 1981; O'Connell *et al.* 1982; Redding 1981). These examples are offered to demonstrate that optimal foraging theory is being consumed and applied in a variety of contexts and is currently having considerable impact on archaeology. An exhaustive critique of optimal foraging theory or even a

reasonable summary of its contributions to archaeology and anthropology is not possible within the scope of this paper (see Winterhalder and Smith 1981). Rather, I will use optimal foraging theory to illustrate some of the general pitfalls that may accompany uncircumspect borrowing.

## THE PROBLEM OF LITERAL BORROWING

The major problem with many applications of exogenous methods and theories stems from their literal transfer from one discipline to another. Nowhere is this problem more evident than in the dependence of archaeologists and other students of cultural evolution on biology for models, mechanisms, and analogies. In a cogent critique, Morales and Levins (1974) caution us regarding our relationship (as social scientists) with the biological sciences, that we should neither borrower nor lender be (see also Curry 1980). Were we to follow their advice, archaeology would be methodologically and theoretically barren. Nevertheless, their critique merits our attention.

There are obvious differences in the *big* questions asked by biologists and social scientists that can largely be attributed to differences in the subject matter, to fundamental differences between biology and culture. Often in transferring models from the natural to the social sciences, similarities between these domains are stressed and differences are trivialized or ignored. Social phenomena are often translated into a biological idiom, and cultural concepts are tortured to fit the biological mold. Hence we see the equation of mutation with invention, gene flow with diffusion, gene pools with arrays of cultural behavior, mating with marriage, dominance with social class, and biological reproduction with social reproduction (e.g., Bar-kow 1978; Feldman and Cavalli-Sforza 1976; Ruyle 1973; cf. Morales and Levins 1974; Sahlin 1976). When social variables such as leadership, social relationships of production, exchange value, and political organization are not molded to fit a biological idiom, they are frequently factored out of the analysis or reduced to residual behavior (e.g., Keene 1981; Winterhalder 1981). This practice may be justifiable as a first approximation (see Keene 1981:13, 233–235) to help us get a handle on the relationships that obtain among some variables; but ultimately what happens is that social causality and social complexity get approximated away because they are outside the set of behaviors that humans share with other animal populations (see Bender 1978; Ekholm and Friedman 1981; Kus this volume; Root this volume). The source of the problem is borrowing without modification and a tendency to adopt rather than to adapt (cf. Winterhalder and Smith 1981).

Because there are differences in the domains of study, the questions asked, and the units of analysis, failure to modify borrowed models can destroy or trivialize the very phenomena we are attempting to understand.

## The Lending Science Sets the Agenda

The lending science usually sets the agenda for the borrowing science, that is, the lending science determines the relevant questions, the relevant units of analysis, and the relevant variables (Morales and Levins 1974). Frequently these borrowed principles are not readily agreed upon within the lending discipline itself, yet they are readily accepted as “fact” by the borrowing science. Let me offer two examples using units of analysis and currency.

### *Units of Analysis*

In evolutionary ecology, the conventional unit of analysis is the individual. The reasons for this should be obvious. Natural selection operates via the differential reproductive success of individuals. Retention of alleles from generation to generation depends on *genetic* transmission from parent to offspring. However, behavioral phenotypes in human populations may be transmitted from one individual to another independent of biological reproduction and genetic inheritance. This is a point frequently lost in optimal foraging studies in anthropology. As Kirch (1980), among others, has noted, because mechanisms of selection and retention among humans are not dependent on biological reproduction, there must be significant differences in the way we study change in human societies as opposed to other animal populations. The analogy of genetic transmission and natural selection breaks down when applied with any precision to cultural systems (cf. Durham 1976). This also raises the question of whether the individual is the appropriate unit of analysis in anthropological studies.

Archaeologists have circumvented this problem to some degree by treating populations or societies as aggregates of individuals (see Keene 1981). But clearly, the problem is more complex than this, and we must consider whether a society is not more than the sum of its parts. One of the most interesting aspects of human society is the social dialectic that exists between the needs of the individual and the needs of society. Maintaining the integrity of the social system (e.g., maintaining egalitarian social relations [see Root this volume]), may necessitate behaviors on the part of the individual (e.g., reciprocity, diminished individuality, diminished initiative, and unequal production) that are at odds with maximizing his/her self-interest. The needs of an individual and a society are not always congruent,

and conflicts between them are an inevitable part of social life. Cultural systems are unique in their ability to adapt to the dialectics of social life by establishing and maintaining social institutions that mediate these tensions. The social demands of surplus production, reciprocity, and information sharing, for instance, are not entirely visible when the individual is the unit of analysis. Social reproduction necessitates more than the reproduction of individuals. It necessitates maintenance of institutions, roles, and ideologies. Again, none of this is clear by looking primarily at the individual. Our continued use of the individual as the primary unit of analysis may be attributed to an inherited convention, to constraints inherent in the lending discipline. This is true not only of the unit of analysis, but also of the currency we use in our optimal foraging studies.

### *Currency*

Every optimal foraging model requires a currency, and the most widely accepted currency in evolutionary ecology is energy. Energy is essential for the existence of organisms; it influences and is influenced by a broad range of behavioral phenomena; it is easily measurable, thus making it more tractable than other currencies; and there appears to be an empirically verified relationship between efficiency of energy capture and increased fitness in some nonhuman foragers (Kirch 1980; Smith 1979; Thomas *et al.* 1979). Hence, it is argued that if energy is not the perfect currency, it is the best measure available. At the very least, it provides a lowest common denominator for quantifying dynamic relationships and a basis for comparison within a complex ecosystem. Yet, it is important to remember that within ecology a good deal of debate surrounds the validity and utility of net energy efficiency as a currency. Whereas it is generally agreed that energy provides an excellent measure for tracking and quantifying the operation of systems (Adams 1981; Smith 1979; Thomas 1973; Thomas and Nydon n.d.; and others), the relationship between energy efficiency and fitness is not well demonstrated, especially in human populations; and many biologists as well as anthropologists are critical of its overuse (Loucks and D'Allessio 1975; Slobodkin 1972; Thomas and Nydon n.d.; and Vayda and McCay 1975).

What actually is the link between fitness and efficiency of energy capture? Is time freed for predator avoidance or for the search for mates a significant consideration in human populations? Do these activities have reasonable analogues in human populations? What benefits accrue for the individual or the group from maximizing the efficiency of energy capture? Human populations have a variety of needs, both biological and social. In the biological realm, it can be argued that in environments where energy is

not limited, the link between efficiency of energy capture and fitness is questionable and that in such cases the currency must consider those factors that are limiting (Keene 1981; Smith 1979). In borrowing from evolutionary ecology, which is genealogically linked with neoclassical economics (Curry 1980), we have adopted energy as a familiar and conventional means of accounting costs and benefits. It may be that in dealing with human foragers, this mode of accounting is inappropriate. I will illustrate this point with two brief examples.

Binford (1979) has noted that maximization of net energy efficiency on an individual basis is highly improbable among the Nunamiut (cf. Smith 1980) because each time a hunter leaves on a trip, he will carry much more food than is necessary for himself. The surplus he carries will be cached either for his use later or, more likely, for use by others. This system of reciprocity requires an energetic input beyond the basic needs of the individual. There is an additional cost here, the cost of maintaining the social system.

Binford's observations of costs can, of course, still be measured in terms of time–energy costs; however, not all aspects of maintaining social relations can be reduced to time–energy accounting. For example, Johnson's (1978, 1982) discussions of the relationship between increasing group size and the formation of decision-making hierarchies implies a question that can not be measured in traditional terms—the cost of maintaining equality. To generalize, let us assume that egalitarian societies maintain equality through social institutions that ensure, among other things, consensus-based decision making and equal access to information and strategic resources. As the size of a group increases, decision by consensus and equal access to information becomes more difficult to maintain (see also Reynolds and Zeigler 1979). Similarly, as group size increases, more information is generated and must be tracked (e.g., information about trade relations, mate exchanges, alliances, as well as subsistence). Johnson (1978, 1982) and others have argued that as information load increases, there is greater pressure to generate hierarchies in which specialists may handle various aspects of information processing. Failure to do so might result in the diminished well-being of the population as a result of failure to track the physical or social environment adequately. There is a cost here that might be measurable in terms of effects on the fitness of the population.

Yet the introduction of specialists to an egalitarian group also introduces social asymmetry. As any student of egalitarian societies knows, much physical and social effort in these societies is directed at maintaining equality. The presence of an arrogant or selfish individual, or even a unique status, can have serious ramifications for the social well-being of the entire group. Hence there exist various leveling mechanisms—institutions that

maintain the status quo. In the scenario above, hierarchization might lead to an increase in the efficiency of information processing and the quality of information management, and there might be “selective” pressure in favor of hierarchization. But it would also extract a potentially great *social* cost, and we might expect social institutions to select against hierarchization. We could track the process of hierarchization with a cost–benefit model using time and energy as measures. Doing so might illustrate the potential penalties to fitness that could result from inefficient management; but it could not consider the basic and important contradiction between efficiency and equality.

I do not argue that the use of energy as a currency is necessarily inappropriate. But there is no reason why energy should be the sole currency we adopt, other than that it is conventional in the lending discipline. If cost–benefit models are to be used, the currency must be appropriate to the questions being asked. Although food preference may indeed be related to time–energy constraints, it may also reflect on the social organization of work. The study of energy alone may trivialize social relationships and the fact that energy efficiency may result in high social costs. Even in ecology, the controversy surrounding energy is far from settled, so it is disturbing to see the proliferation in archaeology of second generation models that accept this conventional wisdom as fact without regard for or even cognizance of the debate or its implications.

Related to this problem is our tendency to prematurely confirm our borrowed models or to reify early applications and make them part of the data base (Curry 1980). Results of early experiments with borrowed method and theory (or early efforts in the lending field) are frequently adopted as fact in second or third generation applications without recognition or awareness of the simplifications, ambiguities, or even explicit fudging inherent to the original work. The assumptions on which evolutionary ecology is based are not all agreed upon within the biological sciences and have been the subject of considerable debate and skepticism, yet they are often cited as broadly applicable and empirically established (Loucks and D’Alessio 1975; Slobodkin 1972; Wiens 1977).

### Optimization as Tautology

Another problem with literal borrowing, particularly of optimization models, is that adaptation is often converted into a belief in universal optimization (Morales and Levins 1974). The investigative framework becomes both teleological (adaptive systems seek optimal levels of performance) and tautological (optimal behavior *is* adaptive behavior; all creatures optimize,

and it is our task to figure out what they are optimizing). Archaeologists working with these methods have tended to find optimization lurking behind every artifact. It is interesting to note the high rate of success we are having in demonstrating that prehistoric people did optimize. The counterinfluences to optimization, which may in fact be the most interesting aspects of the systems we propose to study, are generally not even considered. This leaves no provision for the discovery or explication of nonoptimal behaviors or outright failures in the archaeological record. Although the potential tautological nature of optimization principles were recognized by many of the biologists employing them (e.g., Maynard Smith 1978), the point is frequently lost on second generation consumers. It can be argued that the value of optimization models is not necessarily to prove that organisms or groups optimized (cf. Perlman 1976; Reidhead 1980); rather, the real value comes when optimization is used heuristically as a theoretical baseline against which we can compare observed behavior. The most interesting results may actually be attained in measuring deviation from the theoretical optimum (Cashdan 1983; Johnson 1978; Moore 1981).

### The Question of Scale and Normative Models

Optimal foraging models are normative models, that is, they delineate how organisms should behave under specified conditions. Normative models can be useful research tools, but in using them we may sometimes forget that behavior is variable. We must ask ourselves what exactly our optimal foraging models are measuring and what we wish to measure. Are we examining adaptations to the highs, the lows, or something in between? In borrowing from evolutionary ecology, we have been less than explicit about the scale of our analyses. When looking at time scale it is important to ask, what is the interval over which we hope to assess optimal behavior? Obviously behavior that is optimal in the long run may be suboptimal in the short run and vice versa. Are we concerned with adaptations to long- or short-term phenomena, to local stresses or global events, to stresses on the individual or on the society as a whole? Often these questions go unspecified or underspecified in our work, yet it is critical that they be resolved at the outset. James Moore (personal communication 1982) suggests that there is an irony in our focus on models of short-term, individual decision-making strategies, for it is precisely this class of phenomena that archaeology is least capable of tracking. Archaeology, of course, is best able to deal with behaviors on a large temporal, spatial, and social scale.

Winterhalder (1980), Root (1980), McCay (1981), and others have noted our tendency to obscure questions of scale altogether in our analyses

by applying modal values in our models. Hence, we look not at adaptations to the highs and lows or the range of variability possible, but to averages or most common events. This practice tends to homogenize behaviors and environments and obscures and real variability we hope to understand.

Optimal foraging theory is designed to look at small-scale, local adaptations. It is within this narrowly defined domain that optimal foraging theory is most effective. However, working within this narrowly defined domain will not force us to confront, nor will it necessarily allow us to consider, alternative processes—in particular the social processes that are significant to a foraging economy and an egalitarian lifestyle. If the questions we are asking are concerned with social relations and the organization and intensity of subsistence production we may find optimal foraging theory to be unsatisfactory in terms of scale and the variables used. Of course optimal foraging theory was not designed to deal with social processes, nor can any model account for all axes of variability. Such a reminder might not be necessary if we, as archeologists, pay closer attention to the limitations of our methods before we use them. It is a well-established assumption in archaeological explanation that greater understanding of small-scale processes will ultimately produce a greater understanding of large-scale processes; that is, lots of little pieces of the puzzle will eventually give us the complete big picture. This notion has important implications for the way we define our problems and the way we go about solving them, and I think it helps explain why we continue to work on the same small puzzle pieces. I suggest that our microecological, small-scale analyses are going to be of little help in generating explanations of larger phenomena. This is not an original insight but is, in fact, borrowed from the disciplines in which optimal foraging theory was developed, microeconomics and evolutionary biology. Microeconomics has certainly had difficulty in the past extrapolating to larger macroeconomic questions, and the current active debate about the relationship between micro- and macroevolutionary theory (Eldredge 1981; Gould 1980) suggests that there is a need to decouple the two processes.

### The Lending Science Sets the Hidden Agenda

In borrowing from other fields, the lending science sets the agenda—the questions to be considered. There is not only an explicit agenda, but also a hidden agenda, a historical heritage within a discipline that carries with it well-defined notions of how the world is ordered. In its use of optimization criteria to model neo-Darwinian processes, evolutionary ecology is indebted to the theories and techniques of neoclassical economics. Without resurrecting the multitude of polemics against the concept of economic or rational

person (Godelier 1972; Polanyi 1944; Sahlins 1972), I feel it is important to recognize the historic basis and implications of this link between ecology and economics.

Scientific inquiry, of course, takes place in a social setting. It expresses social ideas and conveys social meaning. All methods applied in the social sciences inevitably express ethical or ideological positions (Harvey 1973; Mills 1959). It has been a constant battle within anthropology to try to rid ourselves of a variety of ethnocentric biases. Microeconomics, as filtered through evolutionary ecology, appears quite value free. Yet such basic assumptions as supply–demand, cost–benefit, utility and marginality are not value-free technical terms but are implicitly embodied in the ideology of the capitalist society in which we exist. These assumptions exemplify certain notions about “natural processes” that are not necessarily amenable to empirical falsification.

Whether this detracts from the heuristic benefits or explanatory power of these models is open to question. It is critical, however, that we at least ask ourselves how a particular array of historical and social circumstances serves to influence what is borrowed, adopted, adapted, distorted, discarded and sometimes rediscovered (Thomas and Nydon n.d.). Lack of this historical perspective can camouflage the hidden agenda. For example, failing to recognize the debt of evolutionary ecology to neoclassical economics, Rapport and Turner (1977) imply that the observation of these microeconomic principles in the natural world suggests they are natural or validated by nature. We must ask whether we can ever hope to do anything but create a reflection of the current dominant political and economic structure in our reconstructions of the past (see also Saitta this volume). As Donald Boy (personal communication 1980) suggests, we are painfully aware of the dangers of extrapolating from the ethnographic present into the past (see Binford 1972; Paynter and Cole 1980; Service 1971; Wobst 1978); we should be equally cautious in our assumptions that prehistoric foragers were consumers in the environmental market.

## OPTIMAL FORAGING THEORY IN PERSPECTIVE

Having raised several cautions about borrowing, I think it is important to emphasize that it is not my intent to ridicule the use of optimal foraging theory or other borrowed concepts. Optimal foraging theory is a valid mode of inquiry. It is also a limited mode of inquiry, and I do feel it is important to recognize its limitations as we should be aware of the limitations of every method or theory we use.

The value of optimal foraging theory to archaeology is best understood

in its historical context. It was not long ago that archaeologists moved from simply describing subsistence remains to attempting to understand the factors that influenced where people lived and what they ate. As archaeologists paid more explicit attention to *behavior* there was a move from the study of individual sites and inventories to the study of subsistence and settlement systems (e.g. Struever 1968; Jochim 1976). It was generally believed (in the late 1960s) that paying greater attention to the distribution, abundance, predictability, and utility of food resources would enable us to understand a good deal of the patterning observed in the archaeological record. Archaeologists also began (in the 1970s) to investigate the etic basis of food selection (Keene 1979) in challenging notions that food preference was based primarily on “irrational” cultural criteria. The investigation of basic microecological questions and the introduction of the necessary method and theory to deal with them (e.g., catchment analysis, gravity models, optimization theory) was a significant development in archaeology, as it marked an explicit attempt by archaeologists to use theory in pursuing behavioral questions (albeit small-scale questions). It also indicates a move to a more explicitly materialist research orientation. Although the emphasis at this time was almost exclusively on techno-environmental relationships, many useful insights were generated.

Optimal foraging theory in particular was and still is clearly attractive to archaeologists for a variety of reasons.

1. It has helped transform our approach to ecology into a theoretically oriented rather than a primarily descriptive one (Curry 1980).
2. It has generated counterintuitive hypotheses that are testable in both laboratory and field.
3. It has allowed archaeologists to think about behaviors that may not be clearly manifest in material remains.
4. It has helped archaeologists generate alternative interpretations for patterns observed in the archaeological record.

Optimal foraging applications in archaeology have generated some useful insights about microecological phenomena. They have promoted a better understanding of the costs and benefits of differential acquisition of different resources, of the advantage of aggregated versus dispersed settlement for efficient foraging, and of the economics of prey conservation or depletion, to name a few. I reiterate that it is not my intent to catalog either the valuable insights or the multitude of abuses possible in the use of optimal foraging theory. Rather, this discussion of optimal foraging theory should make us more conscious of the a priori limitations we place on our work anytime we borrow.

## THE METHODOLOGY OF "MOUTHTALK"

Why do we borrow ideas and methods from other fields? Frequently we view the lending discipline as being more sophisticated or accurate than our own. In a discussion of theory building (or lack thereof) in anthropology, Elman Service (1969) notes that anthropologists frequently adopt models, especially from the natural sciences, for their seeming elegance and because these models may lend a scientific aura of our work. Then, we go out in search of a question in order to apply the models. Service refers to this tyranny of methods, this process of putting the method first rather than starting with a well-defined set of questions, as "mouthtalk."

Mouthtalk has been with us in archaeology for a long time. Archaeologists have frequently been content to take their cues from other fields, to work with well-defined, tractable questions using proven methods. In doing this, we constrain our field of investigation and our imagination. More complex, less tractable questions fall by the way. The dominance of biological logic in archaeological reasoning has tended to obscure important social issues which are crucial not only for understanding the past, but the world around us today. The evolution of social inequality, the restriction of individual autonomy, and the social causes related to these and other events in a changing world are issues not raised by many of our borrowed methods and models. Archaeology is in a unique position to deal with questions such as these. It is really the only social science that can provide a diachronic perspective on social life in precapitalist, preindustrial, preworld system, prestate societies. Yet to do this archaeologists will need a degree of self-confidence and self-determination. The insights can not come wholly from the outside, for the outside deals largely with the modern or historic world in which the influence of the state and a world economy is ubiquitous. To resurrect Binford's challenge, in order to deal with these important questions, we will need to generate some theories and methods of our own.

The real issue is not whether to borrow or not to borrow. The issue in this chapter and throughout the volume is the definition of questions. Our research must begin by defining our questions and their relevance. In the past, we have been less than explicit about what we want to know and why. Borrowing methods and theories from other disciplines may exacerbate the problem, obscuring the central questions and establishing an exogenous agenda. It is my hope that the stigma of mouthtalk and an awareness of the law of the hammer will keep our questions relevant and ever in front of us.

## ACKNOWLEDGMENTS

This chapter is dedicated to former students in my seminar, *Models of Human Subsistence*. Their criticisms provided the impetus for my reflection on my own work in optimal

foraging theory. This chapter is an extension of an earlier collaborative effort with John Curry. We both took our lead from a perceptive critique by Rosario Morales and Richard Levins (1974). Several individuals offered suggestions on earlier drafts. I would particularly like to thank Brooke Thomas, Bob Paynter, Don Boy, Maura Keene, Bill Fawcett, John W. Cole, and Eric Smith.

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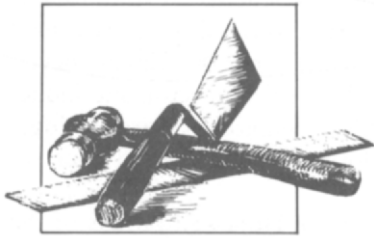
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# 7

## Optimization Models in Context

MICHAEL JOCHIM

Anthropologists working with hunter-gatherer societies have discovered formal optimization models (Keene 1979; Reidhead 1980; Winterhalder and Smith 1981). Such models have been used extensively in other fields, especially economics, ecology, operations research, and anthropological studies of development, and have proved quite valuable in stimulating research. Before these models proliferate in the hunter-gather literature, however, it might be wise to examine their assumptions and interpretations, particularly in light of the growing dissatisfaction with their use in these other fields (Barlett 1980; Gladwin 1975; Johnson 1980; Keen 1977; Maynard Smith 1978; Pyke *et al.* 1977; Rapport and Turner 1977; Winter 1975).

### OPTIMIZATION MODELS: DEFINITION AND APPLICATIONS

Optimization models attempt to explain some aspect of behavior in terms of the maximization (or minimization) of an objective function under

certain constraints. In economics and operations research the focus is usually on marketing or management decisions with the goal of profit maximization. In ecology such models have examined foraging and mating behavior under the assumption of net energy maximization, or time or risk minimization. In each case the models emphasize the exact measurement of money, energy, or time costs of various behavioral options and their evaluation using complex mathematical techniques such as linear programming (Reidhead 1979). The output of these models consists of the least-cost combination of the options available.

In the study of hunter-gatherers, such models are increasingly being applied to the processes of food selection, habitat use, and group formation. These applications use an ecological approach, most frequently emphasizing the adaptive benefits of net energy efficiency. Such models have many attractive features: (1) their objective approach attempts to be unbiased and to allow cross-cultural comparisons; (2) their quantification permits precise measurement of changes in environmental variables; (3) their mathematical complexity seeks to recognize and deal with the complexity of real ecosystems; and (4) for archaeologists working with prehistoric hunter-gatherers, such models have the advantage of allowing the formulation of subsistence and settlement systems as complex hypotheses to be tested against the archaeological record.

Given these advantages, it is to be expected that the use of optimization models in hunter-gatherer studies will increase. These models, however, pose problems of interpretation that cannot be ignored in the pursuit of formal elegance. A clear understanding of the purpose of such models is required before their output may be interpreted. Three possible roles for optimization models will be briefly examined. This examination will emphasize that such models have limited utility and significant liabilities unless attention is devoted to the context of the behavior under investigation.

## **OPTIMIZATION AS A MODEL OF DECISION MAKING**

Optimization models are often phrased in terms of the behavior of the rational decision-maker. Their use in economics has been largely normative, prescribing the best choices among a set of options, given a set of preferences and a goal of minimizing costs. If the actors involved are, indeed, rational, then one might assume that such prescriptive models should be descriptive as well. In other words, optimization models incorporate the same processes of decision making that would be used by a rational economic man. Unfortunately, it has been well documented by studies of real

decision making that the economic man is an ideal (e.g., Tversky and Kahneman 1981). Real decisions are usually shortsighted approximations, characterized by restricted knowledge, faulty perceptions, and limited calculating abilities. The complex mathematical features of optimization models result in a decision-making structure quite different from the actual processes of decision making that people use. Three of these structural differences are particularly important to anthropological studies of decision making.

### Selection of Variables

In an attempt to model behavior such as food selection and land use, ecological anthropologists are increasingly emphasizing such factors as food nutrients and soil chemistry. Although there is no doubt that these variables have important biological and economic effects (and therefore *should* be considered in optimal decisions), they are rarely recognized by the people involved. Counting calories and measuring vitamin C content are unique to modern industrial society, even though calories and vitamins have always been essential for survival. In contrast, people stress other resource attributes such as size, taste, or abundance in selecting foods. Moreover, whereas the models are increasingly striving to include greater numbers of variables, such as multiple nutrients together with costs and risks, people do not tend to consider such huge amounts of information in making choices. On the contrary, people often restrict the information to that considered most relevant and ignore the rest. Among fish sellers in coastal Ghana, for example, the choice of markets in which to sell involves primarily an assessment of conditions of fish supply, ignoring demand almost completely (Quinn 1978). Farmers in some situations assess costs of new crops in terms of cash but tend to ignore labor inputs (Barlett 1977; Gladwin 1979). Nevertheless, despite such evidence that people are poor processors of complex information, ecological anthropologists are using increasingly complicated mathematical techniques to handle all the data that *should* be relevant to decisions (Reidhead 1979).

Because optimal models differ so greatly from real decision making in the definition of relevant variables, the output of the models may deviate significantly from the actual decisions reached. An optimal model of food selection with a goal of satisfying nutritional needs at least cost, for example, might achieve great precision by measuring nutrient contents, mobility, and predictability of various resources. A real hunter, on the other hand, relying on limited calculating abilities, past experiences, and current observations, might find that the size of an animal is a useful guide to the

efficiency of its exploitation—in some contexts. In other situations the herd size of different animals may be a more reliable criterion. Precisely which observable resource attributes assume importance is an empirical question, dependent on many contextual factors such as the nature of the resources and their distribution, the availability of labor, and the procurement technology.

What must be stressed, however, is that the variables used in real decisions, such as resource size, may be only weakly and indirectly correlated with factors of direct adaptive significance, such as energy efficiency. The strength of the correlation will vary with the environmental context. Even if hunter–gatherers in all environments were consciously striving to maximize energy efficiency, varying degrees of success would be achieved. This variation can be anticipated but not yet predicted, and future research must investigate the relationship between emic decision criteria and etic adaptive factors, and how this relationship varies with the environmental context. Certainly, optimal model users cannot simply use one form of model for all contexts, measure the deviations from its predictions, and expect that these deviations accurately reflect something about the basis of resource selection.

Furthermore, it should be noted that people normally seek to attain several simultaneous goals in their behavior. Ease or efficiency of food getting is frequently important, but efficiency of other activities may be an equally significant and perhaps conflicting goal. The use of a few, easily observed variables in decision making, for example, may represent a strategy of efficiency of information processing. It may well be that there is an inverse relationship between the efficiency of information processing about food selection and the precision of measurement (and the resultant energy efficiency of procurement.) The balance between the two goals may be largely dependent on the context of the decisions. As food energy becomes increasingly scarce, as in contexts of population pressure, energy efficiency may be more actively pursued at the expense of ease of decision making (perhaps leading ultimately to full-time decision-making specialists). Most optimal modeling has ignored the costs of decisions, but future research must examine these costs and the contextual constraints on time and knowledge.

### **Scales of Measurement**

The manner in which people measure these variables in order to assess similarities and differences also departs radically from the techniques used in the models. Striving for measures that can permit cross-cultural comparisons, most models employ continuous interval scales such as calories, kilo-

grams, minutes, and acres. In real decision making, on the other hand, people tend to simplify and distort these scales by imposing discontinuities, thereby creating ordinal and nominal scales by means of binary oppositions. Size of animals, for example, might be expressed as a rank order from largest to smallest or even in terms of just two categories, large and small. Moreover, membership criteria for such categories tend to be fuzzy and imprecise.

Such culturally imposed distinctions, discontinuities, and thresholds abound in everyday decision making. The same fish sellers referred to earlier measure market supplies of fish in terms of four categories: glutted, many fish, few fish, and no fish (Quinn 1978). Farmers in Puebla, Mexico tend to determine labor input on a "per plot" basis rather than per acre, even though plots vary widely in size (Gladwin 1979). Thresholds exist in the size of family landholdings in Paso, Costa Rica that serve as important criteria in the choice of land-use strategy (Barlett 1977). The !Kung San recognize a qualitative threshold that marks a shift from 1-day to overnight foraging trips (Lee 1969). These emic distinctions serve as boundaries that strongly influence behavior toward either side of the threshold.

As a simple illustration of the implication of such scales of measurement to resource selection, two hypothetical situations may be considered. In the first case, one may imagine a hunter evaluating two animal resources that are identical in size and nutrient content, but that differ in search and pursuit time necessary for their capture. If one animal takes twice as long as the other to obtain, optimization approaches would lead us to expect that this more expensive animal would figure much less prominently in the diet. If, however, the time costs for the two animals are 10 and 20 minutes, respectively, the hunter might rate them as equal, with both requiring a "brief" search. The etic differences are obscured by the emic scale of time measurement.

One can also conceive of a reverse situation, in which etic similarities are obscured by emic perceptions of significant differences. Again, one may consider two animals, one of which is ten times the size of the other. If this larger animal also takes ten times as long to capture, then the net energy return rates of hunting the two (in terms of kilograms per minute) would be identical. Yet the larger animal might be much preferred because it may be more easily shared, or alternatively, the smaller animal might be given preference because its hunting can be scheduled around other activities in many separate, brief episodes. The important point is that yield may be measured as package size and cost per episode: the continuous scales of weight and time are culturally made discontinuous. The degree of simplification in the measurement of variables may be related to the contextual needs for efficiency of decision making and precision of measurement.

## Combined Assessment of Variables

In the process of resource selection, the variables that are examined and measured must be combined in order to give an overall assessment of each resource, and here again the procedures used in actual decision making differ tremendously from those used in many models. People often make evaluations sequentially and hierarchically, rather than simultaneously, allowing them to restrict further the amount of information to be considered at any one time. The taxonomic keys established by ethnoscientific studies reflect this approach to decision making, in which the native systems of classification demonstrate hierarchical groupings established by an examination of single attributes in sequence, and often in terms of binary oppositions (Berlin *et al* 1973). Anthropological studies of native decisions are increasingly using this approach in an attempt to model actual choice of economic options (Barlett 1977; Gladwin 1979). To refer yet again to the fish sellers of Ghana, the individuals who did consider both supply and demand in selecting markets tended to examine these factors independently and sequentially (Quinn 1978). Hunter–gatherer food selection also demonstrates this sequential approach. Hierarchies of food preferences are often established and secondary “starvation foods” are sought only after examining the condition of preferred foods (Rogers and Black 1976). Again, although greater precision might be obtainable through simultaneous evaluation, the greater demands on time and energy required by such methods might not be warranted. The context of decisions determines both the level of precision necessary and the degree of discrepancy between simple and complex decisions.

## OPTIMIZATION AS A PREDICTOR OF BEHAVIOR

From the preceding discussion it should be clear that optimization models depart radically from the processes of decision making in terms of procedures by which choices are made. On this basis one might conclude that the output of the models—the predicted behavior—will show little agreement with the choices actually made in a given situation. Proponents of the use of optimization models in anthropology, however, may disagree with this conclusion. Specifically, it might be argued that, regardless of the processes by which behavior is chosen, this behavior is subject to natural selection. Since natural selection itself is an optimizing process that maximizes reproductive fitness under certain constraints, selection should produce behavior ultimately in accord with the assumptions of the optimization models (Pyke *et al* 1977). Economists who recognize the imperfections of

human decision making have turned to this argument to support their continued use of such models and ecologically oriented anthropologists similarly stress the adaptive significance of behavior examined in their models. If natural selection is recognized as an important influence on human behavior, then optimization models might be expected to predict behavior regardless of the idiosyncracies and inadequacies of human decision making.

The faith in the predictive capabilities of optimization models rests on three major assumptions. However, each of these is subject to question. The first assumption is that selection operates consistently intensively over a long period on the behavior in question. This has recently been questioned by Wiens (1977), who suggests that the intensity of selective forces may be quite variable, with only certain periods of stress ("ecological crunches") characterized by strong selection. Rather than assuming that optimal behavior will be constantly selected, he suggests that attention be directed to the identification of the penalties paid by suboptimal behavior in various contexts.

It may well be that only in certain situations will such models predict actual behavior with any accuracy. The natural selection of optimal behavior requires a long time-frame, relatively constant environmental conditions, behavioral variation, and observable fitness penalties attached to suboptimal behavior. These situational characteristics should be evaluated before the application of any optimization model so that some *a priori* estimate of the predictive potential of such a model can be made. Natural environments with significant short-term variation and social systems characterized by the buffering of individual failures may represent contexts in which optimization models can be expected to fail in their predictions.

Second, one must assume that optimal behavior is attainable. Human behavior is not infinitely plastic and malleable. Biological models of optimal foraging must assume certain limits to the phenotypes possible because phenotypic expression is constrained by its genetic basis and history; the "best" phenotype cannot necessarily be expected to appear (Maynard Smith 1978:32). Similarly, the "best" decisions cannot necessarily be expected to be reached, particularly if such decisions require complex computational ability and direct measurement of numerous nutrients. Behavior is constrained by its decision-making basis. Hunter-gatherers may have no way of determining precisely that combination of resources that will fulfill their nutritional requirements at least cost. By using computational shorthand and indirect measures, however, they may achieve an adequate but suboptimal strategy. Such a strategy would carry no fitness penalties relative to the "best" possible forager, because this ideal forager would not exist.

A third assumption critical to the belief in the predictive value of optimization models as currently used is that selection will favor energy efficiency.

This assumption is usually justified by indicating the links between energy and biological fitness (Smith 1979). I do not deny the importance of this factor, but it should not be accorded universal primacy as a selection criterion without regard for the importance of the context of adaptation. It is suggested here that other criteria are also important, according to the situation. It should be possible to discover when other factors are scarce and hence potentially relevant to adaptation.

Resource selection, for example, takes quite different forms depending on whether land or labor is scarce. The importance of relative land availability has received wide attention. It is stressed as an important situational variable in studies of agricultural intensification (Boserup 1965; Netting 1969) and population pressure. Yet most ecological models disregard this context and measure costs in time, stressing the importance of energy efficiency per unit time. Certainly, time efficiency is important in production. However, one should more specifically determine expectations about the relative importance of this factor in different contexts, depending on length of the productive season or on the strength of other demands on time.

Land efficiency is also important to varying degrees in different situations. Cree families with exceptionally small territories differ considerably from other families in their patterns of land and resource use (Feit 1973). Comparison of the Mistassini Cree and Round Lake Ojibwa groups studied by Rogers (1962; 1963) reveals significant differences in the use of fish and small game resources. These differences would seem to derive from the nature of environmental conditions encouraging differential emphasis on time efficiency, land efficiency, and risk minimization (Jochim 1981). In an examination of the role of agriculture among the San peoples of Botswana, Hitchcock and Ebert (1980) emphasize the importance of subsistence security as a selection criterion in this environment. For optimization models to predict behavior, greater consideration must be given to the selection criterion, or objective function used in the models. Energy efficiency is not the only proximate measure of fitness, and it may be the most important such measure only in certain contexts.

## **OPTIMIZATION AS A BASELINE FOR THE EXPLANATION OF BEHAVIOR**

It should be clear that ecological optimization models differ drastically from actual decision-making procedures and hence are not likely to predict actual behavior on that basis. Neither are such models likely to predict behavior on the basis of an assumption of constant, intense selection and universal maximization of net energy efficiency. Many advocates of such models would

argue that such prediction is not the models' intent (Keene 1979; Reidhead 1979). Rather, the objective of such model building is often claimed to be the creation of a baseline of behavior to be expected if the underlying goal of the model were, in fact, the goal of the people involved, or at least the effect of the decision-making process. The degree of fit between actual and predicted behavior is expected to correspond to the relative importance of this decision or selection criterion. There are, however, a number of problems with this view.

### Interpreting the Output

The behavior predicted by the optimization models depends not only upon the goals of the model, but also on the procedures of variable definition, measurement, and combination. A test of a model's predictions comprises all of these factors simultaneously. If a linear programming model of time minimization in food selection fails to predict a group's food use with any accuracy, this might mean that the group was not minimizing time costs. It might also mean that the people were trying to minimize costs, but were doing so in their own imperfect way, constrained by their biases and their history, rather than by the complex, simultaneous techniques of evaluation that are characteristic of linear programming. The model's output depends upon its input and its structure, and it must be evaluated accordingly. It is possible that group behavior contrary to the expectations of such a model would tell us nothing about the goals guiding their production decisions or about the nature of selection criteria operating.

### Anchoring to the Model

Despite their emphasis on the objectivity of optimization models, most model builders find it difficult to be objective in assessing the accuracy of fit between their model's output and the behavior being examined. In some cases correctness of fit is evaluated only by inspection. This means that great latitude for interpretation exists, and one of the goals of ecological model-builders must be to develop better tests of agreement. On a more general level, however, *any* model imposes a certain bias on interpretations. Since evidence is examined in light of the model, interpretations become anchored to the model's concepts. Cowgill (1977) has discussed this phenomenon with respect to general significance tests: the null hypothesis forms a baseline for comparison and also an anchor to perception. The evidence is viewed in terms of the null hypothesis and ambiguous cases are perceived as agreeing with it. As archaeologists become increasingly involved in testing

models, this problem of anchoring will have to be recognized. Spatial models, for example, frequently assume discrete levels of settlement size, and so may encourage archaeologists to define such levels in their data. Models of sociopolitical organization often incorporate the concept of stages of complexity and may anchor the perception of past societies to this type of framework.

Optimization models also provide such anchors that will influence the interpretation of behavior. Only adaptive or optimal behavior is modeled, so that maladaptive or suboptimal behavior can only be noted, not explained. Nutritional deficits, such as those experienced by the San with the transition to agriculture (Hitchcock and Ebert 1980) would not be predictable with a model of nutritional optimization. Ambiguous cases will tend to be assigned to a position of agreement with the model, which means that optimal behavior will be found more often than not. One example of this tendency may be cited, not because it is necessarily wrong, but because it illustrates this tendency for anchoring and the problem of testing the model's assumptions.

In a stimulating analysis of subsistence during the Late Woodland and Fort Ancient occupation of the Leonard Haag site in Indiana, Reidhead (1980) uses linear programming to construct a model of nutritional optimization with which to examine food remains at the site. Potential resources are measured for content of 10 different nutrients and for their time costs of procurement and processing. Part of the model's output consists of a ranked list of animal resources that can be compared to the faunal evidence at the site. For the Late Woodland occupation the predicted and observed rankings show virtually no correlation (Spearman's  $\rho = 0.14$ ). Despite this result, Reidhead later claims that the occupants tended toward optimization in their production decisions, that labor minimization was a major goal, and that other, competing demands account for the deviations. This interpretation is clearly tied to the model. Those data in agreement with the model are considered explained by virtue of this agreement. The residue is identified and interpreted in terms of their degree of deviation from expectations.

### Consideration of Other Factors

Human behavior is complex and has multiple determinants. Yet in using optimization models, causal primacy is often given to a single factor such as energy efficiency. In assessing the fit between predictions and actual behavior, all points of agreement are too often considered explained by the model and only discrepancies are further explored. It is not until this stage of the analysis that other factors are considered. Specific cases of deviation

from a model's predictions about food use are frequently explained away by reference to nonenergy factors such as a desire for variety, a quest for prestige, or a need for security or information gathering. Rarely, however, is there any discussion of why these factors should be important in these particular instances. That is, the inclusion of factors other than energy efficiency is on an ad hoc basis, with no consideration of the contextual reasons for their importance. This approach to explanation is similar to the maintenance of a faith in a flat earth, explaining away nonconforming observations one-by-one using whatever factors seem to work. It may be time for a whole new approach. We need to determine the contexts in which factors other than energy and nutrients, such as dietary variety, prestige, security, or information needs, will affect resource selection.

## FUTURE WORK

Given the problems of all models, including optimization models, it is not being recommended that they be abandoned. On the contrary: model building is one of the most valuable and exciting of recent developments in anthropology and archaeology. But since models are essentially elaborate techniques for generating hypotheses and their implications, model builders must accept the widely recognized value of having multiple working hypotheses in order to avoid biases and self-fulfilling hypothesis-testing. Paradoxically, then, the recommendation here is for *more* models to generate more hypotheses for testing.

Any model's output will probably show some agreement with the data, as well as a residue of deviations. When different models are compared to the same data, different patterns of agreement and disagreement will emerge. Any single model, therefore, when considered in isolation, will seem to explain part of the data. Interpretations of this data, consequently, could vary tremendously depending upon one's initial model. Only by using several models simultaneously can we hope to avoid this bias.

If the development of several models is to help avoid biases and anchoring, then the various models should differ by more than the addition or subtraction of a few variables such as specific nutrients in the optimal foraging models. To be useful, competing models should differ in terms of underlying assumptions and mathematical structure as well. A variety of complementary models could be developed, including very simplistic ones, to be used along with the energy optimization models. The emphasis must be on the context of the behavior to be explained.

These complementary models could take a number of forms. The variables included could consist of only those factors that seem generally impor-

tant in ethnographic studies of decision making, such as resource package size, abundance, and fattiness. These subjective variables could still be measured and combined in complex quantitative ways. This was demonstrated in a study by Selby and Hendrix (1976) in which cultural values were used in a linear programming context. Scales of measurement using ordinal or nominal categories could be developed, perhaps (1) by examining modal peaks in attribute distributions; (2) by techniques such as discriminant analysis; or (3) by utilizing threshold values identified in the ethnographic literature. An example of an ethnographically derived threshold would be the basis of site catchment analysis: the 2-hour walking distance from a site. The procedures for combining variables and determining preferences could be modeled using sequential, hierarchical approaches, with either mathematical or judgmental techniques for identifying the criteria to be used at each step of the sequence. Finally, a variety of different goals guiding the decisions or criteria optimized by selection might be suggested, either through an inductive examination of the data or through an assessment of the characteristics of the behavioral context.

A simple example of such a multiple-hypothesis approach may be illustrated by a discussion of Reidhead's data from the Haag site in Indiana. As mentioned earlier, Reidhead's complex nutritional optimization model predicted a rank ordering of animal resources that did not correlate well with the ranked list of the site's faunal data. Even after removing two problematic resources from consideration, this least-cost nutritional model achieved a resource ordering showing a correlation of only 0.50. Rather than accepting this degree of agreement as indicative of a tendency toward optimization, it might first be worthwhile to investigate whether a simpler approach might be more illuminating. One can, for example, examine the role of easily recognizable resource attributes in determining faunal utilization at the site. Two rank orderings for the 13 animals have been established on the basis of their size and abundance, and each ordering was compared to the ranking of these animals by their importance at the site (Table 7.1). The list by abundance produced a negative correlation, but the list by size showed a positive correlation of 0.74. This is an interesting result. It suggests that resource size alone is a better predictor of the hunting preferences of the site's occupants than is the complex nutritional optimization model: the hunters tended to choose large animals over small. Of course, this may simply reflect the fact that large game are more readily preserved in archaeological contexts, but Reidhead (1980) has suggested that differential preservation is not a significant problem at this site.

Such an emphasis on large animals seems to reflect a goal of maximizing the yield per catch, a form of hunting efficiency. Strategies of autumn hunting in high latitudes, for example, might stress such efficiency in order

**TABLE 7.1**  
Rank Orderings for Resources of the Haag Sire<sup>a</sup>

Resource	Rank			
	At site	In Reidhead's model	By size	By abundance
Deer	1	4	3	8
Elk	2	6	1	12
Bear	3	10	2	13
Raccoon	4	7	6	9
Turkey	5	5	5	7
Waterfowl	6	9	9	4
Beaver	7	8	4	10
Squirrel	8	12	12	5
Turtle	9	3	11	3
Fish	10	1	8	2
Mussel	11	2	13	1
Muskrat	12	13	10	6
Opossum	13	11	7	11
Spearman's rho correlation with site ranking		+0.14	+0.74	-0.48

<sup>a</sup> After Reidhead (1980).

to amass food stores for winter. If, on the other hand, the data had suggested an emphasis on abundant animals, this might represent a goal of maximizing the frequency of encountering prey and thus the potential frequency of catches, a measure of hunting reliability. Environmental conditions promoting subsistence uncertainty, for example, might lead to such an emphasis on resource abundance in establishing initial hunting preferences. The context of behavior will be important in determining both decision goals and selection criteria.

Model building must be preceded by an analysis of the context of the behavior analyzed, with a focus on the problems and constraints inherent in this context. Expectations could be developed about the role of specific nutrients in food selection, about the importance of time efficiency to behavior, about the significance of environmental variability and subsistence insecurity, and about the drawbacks of behaving suboptimally. It should be possible to develop a priori expectations about when optimal foraging models could be expected to approximate behavior by demonstrating certain contextual characteristics. These might include long-term environmental stability and clear penalties attached to inefficient behavior, as well as strong correlations among observable resource attributes, their nutritional contents, and ease of exploitation. The temporal dimensions of the context of behavior must be examined as well. Ecologists, for example, often dis-

tinguish between short-term and long-term strategies of behavior, and suggest that fitness is more likely related to the long-term behavioral effects (Pyke *et al.* 1977). We must seek to understand what promotes long-term foraging strategies among hunter-gatherers. One important factor is territoriality, which allows for long-term management of resources. A constant association between people and their resources insures that they will reap the benefits of such management. The lack of fixed territories, on the other hand, tends to promote short-term strategies because people may not be present to benefit from long-term planning. Thus, any conditions that discourage territoriality, such as environmental variability and resource unpredictability, may provide contexts in which suboptimal, short-term strategies would be expected. This is just one example of how contextual factors will affect the predictability of optimal models. Such a priori expectations, together with the use of concurrent, alternative models, should enhance understanding of human behavior and help to avoid the development of scenarios of universal energy optimizers.

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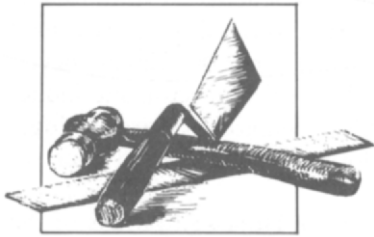
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# 8

## The Trouble with Know-It-Alls: Information as a Social and Ecological Resource

JAMES A. MOORE

Most archaeologists consider society to be an energy-consuming organization. This perspective, by emphasizing the control of energy resources and energy-producing technology, results in a peculiar and limited notion of social power. On the other hand, society can be regarded as an information-processing organization in which social process is driven by the dynamic characteristics of information flows. This chapter addresses two of the issues provoked by this perspective: how is information socially acquired and how is this socially acquired information validated. These two questions lead to new insights on the origins and characteristics of social power.

### THE ARCHAEOLOGICAL CHALLENGE

Far too many archaeologists still believe that knowledge of the human past is transmitted through the artifact and its context. But contrary to our

romantic impulses, the mute stones do not whisper the secrets of the past: the artifacts, their associations, and their distributions will not reveal processes sensitive to variation in flows of information until theories are developed to inform the data (Wilmsen 1980). As long as empiricism and induction dominate both methodology and problem definition in archaeology, little attention will be given to the role of information sharing and information processing in prehistoric social life.

The presence of Yellowstone obsidian in the Hopewell burials of the Ohio Valley prods investigation into the mechanisms of long-distance exchange, and the age–sex distribution of red deer remains at Alpine sites pricks the curiosity about the selectivity of mesolithic hunters in pursuing games. Remains of information flows, however, are not as directly accessible to the archaeologist. There is no empirical puzzle to be found in the archaeological record that directly challenges us to develop a fuller appreciation of the role of information sharing in social behavior.

The lack of empirical puzzles is not simply a characteristic of an impoverished archaeological record. Rather, it grows out of the paucity of meaningful expectations we bring to the data. When our expectations are not met, the puzzle solver in us is challenged to incorporate this new pattern into the old order. When we have no expectations and no theory, we have no puzzles. It is the goal of this chapter to present and develop a few expectations concerning information gathering and information sharing, and thereby to expose some puzzles arising from the interaction of these expectations and our observations of the archaeological record.

## DECISION-MAKING MODELS

It might be said without much exaggeration that the archaeology of the past decade has been marked by the application of a few insightful ideas applied to a wide variety of contexts. One such idea has been the application of individual decision-making models to the anthropological problems of subsistence, settlement, and economic exchange (Coombs 1980; Jochim 1976, 1981; Keene 1979; Reidhead 1980). The broad appeal of decision-making models is not without reason. Decision-making models have provided the mechanisms linking the flow of matter, energy, and information to the operation of cultural systems. The grand theoretical perspective of a cultural systems approach (Adams 1975; Flannery 1972; Miller 1978) generated exciting new implications for archaeology. Yet for all its success, it is now generally admitted that the strength of the cultural systems approach rested more on its metaphorical qualities than on its formal logic (Flannery 1972; Rappaport 1968; Wright and Johnson 1975). Promotion, lineariza-

tion, centralization, and segregation are powerful lenses for focusing research, but they are also only rich illusions that fail to explicitly specify processual mechanisms.

For many archaeologists, the lack of a formal logical connection between the behavior of a cultural system and the behavior of individual human agents was seen as a formidable theoretical problem. Attempts to explicate cultural process at the systems level generated few insights as to how individual participants sense the constraints of linearization (or any other systems pathology), how this leads to the modification of individual behavior, and how this aggregate of individual actions becomes an effective response to the pathologies of the cultural system (Friedman 1979).

In the last decade, the formal recognition that decision-making models can link the behavior of individuals to the operation of the cultural system spurred a flurry of research aimed at investigating the implications of various decision-making strategies. The study of hunter-gatherer subsistence activities has been totally transformed with the application of ecological optimization approaches (see Jochim this volume; Keene this volume). In a similar manner the application of optimization criteria to agricultural societies has led to a profitable redefinition of subsistence and settlement problems (see Cancian 1972; Flannery 1976; Higgs and Vita-Finzi 1972; Jarman 1972; Peet 1969).

## THE LIMITS OF INDIVIDUAL DECISION MAKERS

Optimization models have successfully organized an impressive diversity of subsistence, locational, and economic behavior in a coherent framework that informs our observations of many different societies. Yet it is worth noting that optimization approaches to individual behavior are models, and like all models, they make the complexity of the world understandable by presenting us with simplified and incomplete versions of that complexity. It is important for us to understand the cost of this simplification and the necessary distortions that accompany any simplification.

The original attraction of individual decision-making models was their ability to provide mechanisms through which we could understand the flows of matter, energy, and information through cultural systems. However, in using these models, we have reduced complexity by ignoring the flow of matter and information in order to better understand the factors that influence the regulation of energy flows. We now better understand the dynamics of energy flows, but only at the cost of having removed it from its cultural context. Our decision-making models are successful, but they are only first approximations.

The problems addressed by individual decision-making models depend entirely on the assumed characteristics of the decision maker. It is not difficult for a researcher to tailor the decision-making model by modifying the goals and criteria of the decision-making process (Moore 1981), however, most investigators have decided to use the abstraction of economic rationality as a conventional and convenient standard. An all-knowing, computationally perfect decision maker serves a useful heuristic function by holding the goals and criteria of the decision-making process constant while exploring the effects of changes in costs and benefits (Pred 1967; Salop 1978). This simplifying assumption has permitted us to explore the intricacies of economic, geographic, and ecological models in a variety of anthropological contexts (Durham 1981; Winterhalder 1981). However, the simplifying assumptions granting omniscience, errorless and infinitely rapid calculation abilities, as well as freedom from social or cultural constraints to the decision maker, elevate cost-benefit evaluation to the position of a cultural universal. They rob our decision makers of any social or cultural context. As a result they create an impoverished base from which to begin any investigation of the relationship between intentional individual actions and their effects, sometimes unintentional, on social organization. Rational economic decision-maker models assume that all individuals evaluate costs according to the same cultural values and weigh benefits according to an ideal set of needs (Jochim this volume). Omniscient decision-making models transform social relations into economic relations and limit our understanding of social process to an understanding of modern economics.

The elegance of this economic formalism has led us to expend a tremendous amount of research energy on the intellectual equivalent of the drunkard's search. Just as the drunk searched for his lost key under the streetlamp because that was where the light was the brightest, archaeologists applying individual decision-making models have used rational economic decision-maker assumptions because the formal and mathematical logic has been worked out to the greatest detail. And, again, just as the brightest streetlamp lit the area where the drunkard did not need to search; the rational economic decision-maker assumptions have helped us answer questions that do not advance us toward our ultimate goal of understanding the regulation of matter, energy, *and* information flows, as well as the long-term transformations of social forms.

The inadequacy of omniscient decision-making models is further highlighted by the fundamental contradictions involved when optimization is attempted under conditions of competition (Pred 1967:7-12). First, those specific actions that will actually be optimal for any given individual often do not depend solely on the characteristics of the individual, but upon the actions of the competitors as well (Simon 1959:266). And if optimality

hinges on the predictability of competitors' future actions this in turn means that optimization requires every decision maker to outguess all competition without being outguessed by them. Framed in this manner, the problem of optimization under competition rapidly gains complexity until it approaches the intractability of the prisoner's dilemma (Rappoport 1960). Although optimal outcomes are possible for some skilled and lucky individuals, this outcome cannot be extended to the entire population of economic decision makers.

The second logical problem involves the issue of the temporal scale over which the decisions are to be evaluated. Is optimization to be sought over the short run or the long haul? But once again how long a decision remains an appropriate strategy does not depend solely on the actions of the decision maker, but also on the characteristics of the decision-making environment. The actions of competitors and unforeseen changes in social and technological conditions can completely upset the elaborate calculations of the rational economic individual. Decisions that involve large investments of time and energy in the construction of facilities, also generally imply long-term commitments to that strategy in order to recoup these threshold costs. A decision maker's uncertainty about future conditions will limit the extent to which strategies involving investment in facilities are selected.

Furthermore, once these calculations incorporate a probabilistic element, the *mean expected value* criteria often used in economic decision-making models can be inappropriate. The mean expected value criteria selects the optimal long-term strategy. Yet unless the decision makers have the ability to withstand unavoidable short-term losses, this criteria can result in intermittent disasters. When strategies can be shifted with little cost to the decision maker, this may be a mere inconvenience. But as the cost of strategy shifting increases, decisions made on the basis of the mean expected value criteria may be optimal over neither the short nor the long term. Such considerations are generally ignored in anthropological applications of decision-making models (Vayda and McCay 1975).

The final difficulty with the rational economic decision-making model is the assumption of what Simon (1957) has called "preposterously omniscient rationality." This is one of the hammers I see being swung most wildly in anthropological archaeology. With the widespread adoption and application of decision-making models, omniscience has become one of those invisible assumptions that mark the practice of normal science. By circumscribing the scope of scientific discourse, the assumption of omniscience has permitted us to understand how ecological and spatial contexts influence the flow of energy in a society. However, at the same time, it removes the informational context of decision making from the realm of anthropological research. The assumption that societies are no more than groups of know-it-

alls directly eliminates the potential that information-flow models hold for the study of social organization.

### Information As a Resource

Information is a resource. It is something that must be gathered either directly through searching the environment or indirectly through the social processes that distribute information among the individual decision makers. It is an *oversimplification* to ignore, as economic decision-making models do, the social factors that influence an individual's access to information. Quite simply, information is not free in terms of acquisition costs, it is not distributed evenly over the social or physical environment, nor can it be obtained without creating constraints on the range of future actions (March and Simon 1958). By assuming that decision makers have perfect knowledge, we have failed to acknowledge that societies are organized by access to information as well as by access to matter and energy (see also Root this volume).

## THE ACQUISITION OF INFORMATION

From its earliest days, anthropology has shown an interest in the issue of information acquisition. The concepts of diffusion, socialization, and acculturation were developed to deal with the issues of how individuals acquire the cultural practices of their own and other groups. Early concerns were directed toward learning normative rules, and understandably, the concepts lacked a space–time framework. Initially, little concern was shown for how distance affected diffusion or for how the duration of contact influenced acculturation. Similarly, anthropologists simply applied the label “socialization” without developing or investigating the mechanisms behind it. Without the development of mechanisms, anthropologists found themselves asserting that information is distributed within and among societies whenever conditions permit. The failure of diffusion, acculturation, or socialization to occur simply indicated that conditions did not permit the operation of these processes. These notions of information acquisition are merely flimsy tautologies incapable of leading to any understanding of information distribution processes.

The spatial implications of diffusion have, however, been dealt with in geography where diffusion has been used to study migration, settlement, and the spread of innovations. Among the most influential of these studies have been those carried out by the Lund School of geographers founded by

Torsten Hagerstrand. The foremost characteristic of Hagerstrand's work on the diffusion of innovations and patterns of migration is his ability to disaggregate normative generalizations into patterns of individual behavior. His Monte Carlo simulation models of the spatial diffusion of innovations rest on two simple but powerful principles. First, he states clearly that the acceptance of innovation is dependent on the receipt of information, and the receipt of information is in turn inseparable from the spatial characteristics of the private information fields of individual decision makers. Information fields with long-distance contacts among individuals transmit information about an innovation over a much broader area than compact information fields. However, the acceptance of innovation does not simply rest on the calculation of economic cost-benefits by the recipient of the information. Hagerstrand posits a second behavioral attribute of the diffusion process: validation of the original information is required. Hagerstrand believes that "a person becomes more and more inclined to accept an innovation the more often he comes into contact with other persons who have accepted it" (Hagerstrand 1953:266). In other words, information concerning an innovation acquires increased validity after it has been received from several different sources.

These two simple behavioral characteristics combine to form a mechanism for carrying out the diffusion process. But even more impressive is the fact that by varying the characteristics of the private information fields and the levels of uncertainty reduction, Hagerstrand varies the spatial patterning of the diffusion process. Compact information fields and situations where few contacts are required to reduce uncertainty result in the familiar wave-front model of diffusion, whereas widespread information fields tied to high uncertainty reduction characteristics lead to an almost random looking patterning of innovation acceptors over the region. Although his study deals specifically with innovation diffusion, the results of Hagerstrand's simulation experiments can be generalized to any situation involving the spread of information over a region.

Hagerstrand's (1957) investigation into the patterns of Swedish migration are also based on behavioral assumptions. In comparing the migration to the Swedish cities of Norrköping and Linköping, he found that the smaller city showed a greater influence on the patterns of migration than predicted by a gravity potential model where population size and distance determine the relative frequency of migration. By again separating generalized aggregate behavior patterns into the behavior of individuals, Hagerstrand discovered that the migrant's knowledge of job opportunities and his acquaintances with previous migrants accounted for the movement patterns. The spatial pattern of the migration process reflected the aggregate pattern of individual decisions where the decisions could be explained in

terms of the distribution of information flow through a social network of interpersonal communication. Distance, the determinant factor in gravity potential models, was found to be merely an operational proxy for the more behaviorally salient characteristics of the individual information field.

Hans Aldskogius (1969) elaborated on Hagerstrand's thinking by including the development of social networks in his model of the evolution of Swedish summer home communities. Aldskogius found that early summer house builders were heavily influenced by the locations of bus terminals, train stations, and hotels. Areas near these facilities were more easily explored, and the summer house locations more easily discovered. Once the first few summer homes were built in these areas, the spatial bias of the individual information networks were well established. The resulting spatial distribution shows clusters of summer houses widely spaced on a region of otherwise equally attractive potential locations. The implications of Aldskogius's study are particularly rich for the archaeologist. It is not the ecological requirements of summer houses—undeveloped waterfront, mature forest, absence of industry—that lead to the clustering. Clusters resulted from the infrastructural requirements of exploration and the spatial bias of individual information fields.

From these few studies it is possible to see that the notion of the private information field offers a mechanism for explaining the acquisition of information. However, in archaeological contexts, the concept of private information fields presents a challenge. For the geographer, the size of the private information field is an empirical issue. It is measured directly through interview and observation techniques, and there is little need for understanding the factors that determine its extent and density. This lack of theoretical understanding is a problem for the archaeologist who cannot treat the size of the information field as an empirical question.

## INFORMATION FIELDS IN ARCHAEOLOGY

The notion of an archaeological culture as indicated by recurrent assemblages has often been assumed to be the material manifestation of an ethnographic culture (Clarke 1968; Deetz 1967; Willey and Phillips 1958). Clarke (1968:358–398) and others go so far as to relate the presence of similar artifact forms to the rates of social interaction, and to relate changes in assemblage similarity to decreases in social interaction. Most archaeologists accept implicitly the notion that archaeological cultures represent information fields through which material culture and social values are shared. From Clarke's perspective, cultural boundaries are merely the results of decreasing interaction and information exchange. It is important to

note here the direction of the causal arrow: decreasing interaction leads to cultural boundaries (see also Cross this volume).

The relation of interaction and cultural boundaries creates a starting point for the archaeological study of information fields. If it is possible to identify a minimal social interaction field we have also identified the minimum individual information field and the minimal size of cultural entities. This is precisely the logic that guided Wobst (1974) in his demographic simulations of paleolithic society. By defining minimum equilibrium population size, Wobst determined the minimal social field of interaction in paleolithic society in order to establish a social context for individual sites. The important point in Wobst's model is that the demographic characteristics of paleolithic society require that each individual have a private information field of roughly 350 individuals.

Originally, Wobst (1974) implied that this group of 350 was likely to be identifiable as a cultural group. Later, however, he pointed out that his simulated mating networks, and hence private information fields, overlapped extensively, creating a continuous chaining of mate and information exchange across the region (Wobst 1976). He concluded that at low population densities, closed mating networks are unlikely, and thus cultural boundaries are unlikely to form.

Additional studies by Barth (1969), Hodder (1981), and Wobst (1978) bring the notion of cultural boundaries into question. Cultural boundaries do not necessarily represent a decrease in social interaction rates. As Hodder (1981:92) states, "The structure of cultural differences and similarities thus depends less on the intensity of interaction than on its nature." In other words, cultural boundaries are not caused by decreases in interaction, but by changes in the types of interaction and the types of information passing through the boundary.

## **SOCIAL ASYMMETRY AND THE EXCHANGE OF INFORMATION**

In the preceding discussion it has been implicitly assumed that the private information field is spread through a homogeneous network of social interactions, and the type of information received from all sources is equivalent. This is a reasonable simplifying premise. Essentially, it assumes that any two individuals are capable of fully transmitting the information they control to each other.

How can we explain changes in the nature of social interaction and limits on the types of information that any two individuals will exchange? From the standard anthropological perspective, this problem is easily solved

by pointing out the existence of social groups and social status as leading to the differentiation in the patterns of information exchange. However, this is simply a typological assertion. The problem is merely shifted to the question, Why do social status and social distinction arise within and among groups?

The differentiation of private information fields has been addressed by Johnson (1978, 1982). The fundamental problem limiting the extent of private information fields is information overload. Johnson argues that as the number of information sources increases there is a rapid increase in the costs of monitoring and processing the information, resulting in pressures toward either hierarchical (1978) or horizontal (1982) segregation of the individual's information field.

Hierarchical segregation of the information field involves the creation of information processing specialists. In the simplest situation this involves the centralization of information in a single social status. The person in this status then becomes the agent for the dissemination of information. Ethnographically, this situation can be recognized in Steward's account of the Shoshonean "talker" (Steward 1938:427):

Many of the larger villages, however, had a single headman. His title means "talker." His task was principally to keep informed about the ripening of plant foods in the different localities, to impart this information to the villagers and, if all the families travelled to the same pine nut area, to manage the trip and help arrange where each was to harvest.

In the simple case, the information field of the talker does not include many other talkers. But if the information fields of these first-level information processors overlap extensively with other information distributors, information overload can once again occur, creating pressure toward the formation of a "talker's talker." In this manner, Johnson sees the evolution of hierarchically organized information fields able to coordinate a vast number of information sources while not overloading the information handling abilities of the individuals involved.

It is important to note here that only certain types of information will be transmitted through this hierarchical organization. Among the Shoshoneans, the talker was only responsible for subsistence information and locational plans. While acting out the role of talker, there is a qualitative change in the type of information that is distributed. In other words, status positions identify the type of information an individual in that status will receive, and the sorts of information that individual will have for distribution.

Horizontal segregation of the information field also provides a means of expanding individual information fields while containing information processing costs (Johnson 1982). Whereas hierarchical segregation de-

creases information-handling costs by structuring the flow of information among individuals, horizontal segregation lowers information-monitoring costs by creating socially equivalent individuals. In lumping individuals together into clones of a single social persona, it becomes possible to gather information known to every individual in that group through monitoring a single individual in that group. This strategy is possible, however, only to the extent that every individual in the social group shares the information needed for exchange between differing social positions. Since this condition only exists for certain limited types of information, again, only partial information would be transmitted across the boundaries marking social positions. In this case, membership in a social group identifies an individual as a source of certain types of information, and the nature of the social interaction is accordingly standardized.

Johnson's arguments indicate that social organization is at least partially determined by the dynamics of individual information fields. Social status positions and social groups organize information pooling and thereby create channels through which only certain types of information are exchanged. The concept of the individual information field that originally assumed dyadic exchange among equals has grown in complexity as we have added asymmetry to the nature of information exchange. In addition, a new tier of information has been generated and added to the original information concerning the distribution of resources, consumers, and mates. As the variety of information sources and destinations increases, it is necessary for an individual to be able to identify the individuals now filling these social positions. This is a qualitatively new type of information—information created by the social organization that has no referent outside the social framework.

## THE VALIDATION OF INFORMATION

Once we transcend the idea of the omniscient, rational decision-maker and place information in a social context, we must confront not only unequal access to information, but also unequal quality of information. In reality, decisions are based on a mix of information, ignorance, error, and lies. Moving beyond the comfortable first approximation of perfectly distributed information will necessitate an understanding of the complex behavior involved in information validation. To remove the issue of information validation from the domain of research—as the assumption of omniscience has done—creates a social world ignorant of the fundamental human characteristic of communication and all the social problems associated with imperfect communication.

Information is a resource, but it is a queer sort of resource. Individual attempts to perfectly monitor the ecological environment can be very costly in terms of time and energy. Yet the information acquired can then be shared among an entire group without diminishing the amount of information held by the original collector. Thus, the costs of replicating information are low. However, there is a complicating factor. The costs of replicating false or out-of-date information are also low. This means that the information flowing into any individual's information network will contain a combination of useful and misleading statements. Because of the necessity of validating socially acquired information, our models of reciprocity, redistribution, and market exchange are not likely to replicate the processes that distribute information. Once we consider the issue of validating socially acquired information, the process of decision making becomes a much trickier business; but the solution to this problem becomes much more socially interesting.

The flow of information through an individual's information field is socially organized. Therefore, information is not intelligible outside the context of social organization. Furthermore, specific information cannot be acquired until the individuals filling the social position controlling that information can be identified. Material culture provides a necessary means for broadcasting social identity (Conkey 1980; Hodder 1981; Plog 1980; Wobst 1977), and hence, the social location of information. The seminal study by Wobst (1977) points out the three basic premises of this approach. First, style has a heavy information content—only a few bits of information are transmitted; however, these bits are heavily invested with meaning. By implication, style will not carry trivial information. Second, the broadcasting strength of the stylized items is directly related to their visibility, both in terms of context and size. Third, the broadcast strength of the stylized item is directly related to the social importance of the information. In the ethnographic case study of Yugoslavian ethnic groups, Wobst demonstrated that the broadcast strength of the stylized items directly related to a hierarchy of we—they distinctions. As one moved from ethnic identity to village affiliation to social position, the broadcast strength of the styled item diminished.

Hodder (1981) reaches similar conclusions in a study of Zambian ethnic groups. The social boundaries among the groups are not signaled by the presence of different material assemblages, but rather by the existence of a few material identity markers. These markers do not signal the degree of cultural difference; rather, they identify particular sorts of social relations. Material culture is used "to support within group cohesion, coordination and self interest and to justify between-group rivalries" (1981:92). Hodder (1981:95) concludes, "it is not possible to discuss stylistic variability with-

out taking into account social relations and tensions and the symbolic nature of material culture.”

In a provocative study, Macdonald (1980) examines the hypothesis that style carries a social message. Among horticultural groups in the northern Luzon region of the Philippines, the stylized shapes of each warrior's shield are normatively associated with group membership. During ambushes a warrior could rapidly scan the distribution of shields to check the position of friend and foe. So far these results match the expectations of the Wobst style model. However, the case is not as simple as it first appears.

Macdonald found that warriors would often engage in treachery by taking up the shields of fallen enemies. This permitted attacks from the side or rear that often went unnoticed until the apparent ally turned on his victim. Lying with style is both possible and effective. The groups responded by increasing the redundancy of the communication system. The symbol was transferred from the shield to the warrior himself. Full body tattooing duplicated the symbolism in a manner that could not be easily falsified by enemies. By increasing the cost of creating a stylized lie, the truthfulness of the message was guaranteed.

Although Macdonald's analysis points to the importance of redundancy in validating information, examining the nature of communication itself produces some insightful distinctions. In its simplest form, communication involves the encoding of a message, the transmitting of the coded message along a channel of communication, the reception of the coded message, and the decoding of the message. When discussing communication, it is often overlooked that for communication to be effective the sender and the receiver must share the same code. With the social differentiation of the quality of information exchanged, there is often a change in the codes used in communication. Until the sender and receiver can establish the code to be used in communication, the information exchanged will be ambiguous. What is required to solve this problem is a metacode—a standardized code identifying the code and the channels of communication to be used to exchange information. This metacode is a second tier of information. It is a communications code that has referents only to the social status positions and social groups in the information field.

It is important to note, however, that this code does more than simply identify the lower-order code to be used in communication. The knowledge of the code both validates the receiver's access to the information, and it identifies the sender as being a legitimate source of the particular sort of information being transmitted. Most socially transmitted information is validated in two manners. First, information is evaluated in terms of the code in which it is transmitted. For instance, in the United States, a Bronx or Southern accent may be identified as an uneducated code, and the informa-

tion received from such a source is often discounted for that reason. Second, information is evaluated according to the sender's ability to use the meta-code to identify himself as an appropriate source of the information transmitted. By validating the source, we validate the information.

Macdonald's study of shield shapes and full body tattooing demonstrates the usefulness of these distinctions and the social dynamics that they uncover. The shield's shape serves as a metacode, it identifies the warrior's group independent of any spoken communication. But interestingly enough, it is possible to lie with the metacode as easily as it is to lie with language. The fully tattooing offers a validation of the metacode—the warrior has the tattoo that identifies him as an individual who can legitimately carry that shield.

Material code transmitters are used to verify the social identity of individuals and as a result, they tend to establish the social context in which the message is to be understood. Knowledge of the codes permits the actor to present or misrepresent his social, economic, and political position in the social order (as Vonnegut has noted, we are who we pretend to be). And because controlling codes can give one access to matter, energy, and information, it is likely that further stylistic elaboration will develop where an asymmetry in the information exchange can be converted to social position and social power. Elaboration of the second-order codes sets into motion a powerful social dynamic.

The implications of the preceding discussion for interpreting patterning in the archaeological record should be clear enough. The need to validate information and the social position of the source of information has implications for stylistic change, exchange of exotics, and the development of esoteric knowledge. For example, exotics can validate an individual's claim of long-distance ties, esoteric knowledge can validate group membership (clans, secret societies, priesthood) (Peebles and Kus 1977), and stylistic change can present evidence of code busting (Rathje 1972).

The control of codes and metacodes can both maintain and create social positions. Information is a valuable resource, and metacodes provide a key that permits access to both additional information and noninformation resources. Metacodes, and the social relations they create, are a source of social power. The struggle to control the metacodes gives birth to a social dynamic that is invisible to calorie-counting omniscient decision-makers.

## CONCLUSIONS

Archaeological research of the past 20 years has been dominated by the notion that human behavior can be explained in terms of spatial and tem-

poral distribution of resources. Maximization and optimization approaches were developed to provide mechanisms whereby human decision making and hence human behavior were determined by spatial and temporal abundances. Underlying these models was an important but often implicit assumption. In our models, hunter–gatherers, early agriculturalists, and early state bureaucrats were all omniscient.

The success of this orientation has blinded us to the fact that often neither food energy nor raw materials limit human behavior. Information is an important resource, and both the lack of information and the inability to process and distribute it constrain behavior. Regional settlement densities may be limited not by the density of food energy, but by ignorance and lack of coordination of seasonal settlement activities (Moore 1981). Seasonality is an attribute of the ecosystem, but scheduling is a social phenomena. This first tier of information has an obvious and direct effect on how humans respond to temporal and spatial variations of food resources.

There is also a second tier of information, having a much more subtle, but dynamic influence on human behavior. The very nature of information exchange creates pressures to organize and structure the flow of information. This pressure exists independent of the specific type of information exchanged. Once an information flow is structured, much of the information concerning distribution of energy and consumers becomes unobtainable unless indexing information—information about the location of information—is distributed.

Indexing information can easily be used to create asymmetrical social relations. The social power inherent in possessing indexing information can precipitate struggles for its control. Competition, cooperation, collusion, and duplicity are all social strategies that can be brought into play as individuals and groups seek to gain control of this valuable resource. Furthermore, these dynamics take place without any *physical* control of food or raw materials.

This has been, in many ways, a prefatory discussion. I have neither presented a well-documented case study nor grounded the discussion in archaeological data. To stress these shortcomings, however, ignores the central issue that I have addressed—the social nature of social power. Omniscient decision-making models have helped us develop an understanding of the mechanisms that regulate the flow of matter and energy through a cultural system. Omniscient decision makers are and will continue to be important tools in archaeological research. But they become hammers when we use them to address issues of social organization, or when their results are milked for implications about social power. Information models, concerned with the acquisition and validation of information, are far richer in implications for social organization, and indicate more about the nature of

social power than our omniscient and unintentionally egalitarian decision-making models.

## ACKNOWLEDGMENTS

I would like to thank Dolores Root, Martin Wobst, Robert Paynter, Kate Pfordresher, and Maura Keene for comments on an earlier draft of this paper.

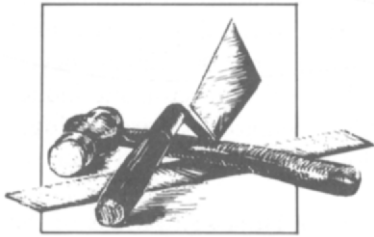
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# 9

## Information Exchange and the Spatial Configurations of Egalitarian Societies

DOLORES ROOT

### INTRODUCTION

The potential of settlement pattern studies to provide insights into social process has been and is still broadly embraced by archaeologists. The attachment to settlement studies reflects a conviction that culture process is revealed through settlement data. Thus, attention has focused on collecting settlement data and improving data collection methods.

However, the goal of settlement pattern studies to generate explanations of culture process, remains relatively elusive. The failure does not lie with our methods as much as with our failure to design research questions so that variables sensitive to social process are specified. By social process I mean the internal organizational dynamics of cultural systems; in other words, the dynamic relationships that produce, among other things, the spatial configuration of settlements.

This relationship between social organization and the spatial order should be tractable through settlement data. I will examine some of the components of this relationship with respect to egalitarian societies. The problems addressed are twofold. The first problem is to identify the social processes that maintain the organizational structure of egalitarian societies. Egalitarian systems occur in a wide variety of social, economic, and ecological conditions. The social processes that maintain equal access to critical resources under these varied conditions are not apparent. The second problem is to determine the material correlates of these behaviors and to generate expectations for their occurrence in the archaeological record as well as implications for the spatial configurations of settlements.

As background, I will consider how archaeologists have studied the relationship between social organization and the spatial order through settlement studies, and we will discuss some of the limitations of these approaches for understanding social process. Then an alternative approach is proposed using social relations of production as an axis of variability allowing us to study social process. I examine how the social relations of production are manifested in egalitarian societies and mediated by information. I develop the material correlates of information flows in the archaeological record and derive the implications of these processes for settlement studies and for identifying variability in the spatial configurations of egalitarian societies.

## THE SETTLEMENT APPROACH

The development of settlement studies represents a significant shift in the focus in archaeology away from the artifact as the primary unit of analysis and away from chronology and classification as the goal of archaeological studies. Willey (1956:1) viewed settlements as a "direct reflection of social and economic activities," and he proposed settlement studies as a "strategic starting place for the functional interpretation of archaeological cultures" (Willey 1953:1). Implicit in this approach is a concern for the relationship between sociopolitical organization and the spatial configurations of settlements. Generally, it is expected that culture process can be identified by tracking the arrangement of settlements over space and the change in their distribution over time. Sociological explanations for the spatial arrangement of settlements are often inferred on the basis of ethnographic analogy (Campbell 1968; Chang 1958; Clarke 1976; David 1973; Jochim 1976; Thomas 1973; Vogt 1956), or by reference to cultural evolutionary models (Beardsley *et al.* 1956; Parsons 1972; Ritchie 1969; Sanders 1956; Sanders *et al.* 1979; Willey 1953).

Over the past 30 years we have seen a florescence of methodological approaches to settlement data. Many of the methods initially developed as “experiments”—ways to approach settlement data, often borrowing from other disciplines. With time they have become accepted procedures for settlement pattern archaeology even though the assumptions on which they are based are often unjustified or untested.

The proponents of settlement studies have advocated settlement data for understanding cultural process (e.g., Chang 1958; Struever 1968, 1971; Trigger 1968; Willey 1953). However, the view of culture process that has emerged with the various approaches is narrow. In the majority of the cases, process is limited to a description of static patterns, so that the “dynamic relationships (causes and effects) operative among socio-cultural systems” (Binford 1968:14) are neither isolated nor specified (e.g., Chang 1968; Fitting 1969; Ritchie 1969; Ritchie and Funk 1973; Willey 1953, 1956; Winters 1969). As a result, culture process studies are reduced to the cataloging of patterns in the spatial and temporal distribution of material culture. Occasionally, methods are applied that are based on a dynamic understanding of culture process (e.g., Struever 1968, 1971), but a parochial view of process persists because of problems of scale or because variables move in one direction only.

### The Settlement as the Unit of Analysis

There has been considerable discussion regarding the appropriate unit of analysis for understanding the dynamics of the configuration of settlements (Binford 1964; Chang 1968; Flannery 1976; Hill 1968; Longacre 1968; Struever 1968, 1971). Chang (1968) advocated the community as the basic social unit and the settlement as its locus. For Chang the social and cultural activities “within” the settlement and “beyond” the settlement are autonomous. His approach to the study of culture process remains typological. He first describes and classifies settlements in order then to determine genetic or generic relationships among groups or to establish culture histories (Zimmerman 1977).

Chang’s logic, implicitly or explicitly, has been the point of departure for a number of approaches designed to study settlement patterns. Site catchment analysis exemplifies these approaches. (Roper [1979] provides a comprehensive review of its methodology and applications.) The basic premise of site catchment analysis is that distance to resources determines site location. Thus, the function and location of a settlement is understood in the context of the local environment, whether it be a 12-km radius as defined for hunter—gatherers (Lee 1969) or a radius with a 2-hour walk as

the determinant of its maximum, as estimated for agriculturists (Chisolm 1968). Consideration of the relationships within a settlement system focuses on the potential resource differences among the sites. Furthermore, a settlement system tends to be defined on the basis of a few "representative" sites that encompass the annual subsistence round.

With the settlement as the unit of analysis, the dynamics of a cultural system are difficult to fathom. The articulations between the local level and the larger system become especially obscure. This is a serious problem, since at all levels of sociocultural complexity the arena of social interactions extends well beyond the settlement and the catchment area (Blanton this volume; Fried 1975; Johnson 1977; Moore 1981; Paynter 1980; Wobst 1974, 1976). Referring to site catchment analysis, Flannery (1976:117) states that it "may help us to determine the location of a settlement, but the social factors which affect the spacing of settlements, or how settlements are integrated into a socio-political-economic system are not explained." Resources or other local level variables account only for local variability and not for the processes that distribute a population over a regional landscape. Studies whose point of departure is the socially isolated settlement cannot provide generalizations about the social processes that affect the organizational structure of settlement systems.

### **Settlement Patterns**

The objective of settlement pattern studies is to delimit regional patterns of site distributions in terms of location, number, size, and function. Culture change is tracked through changes in site distribution patterns between culturally defined time periods. It is assumed that the changing patterns of human activity reflect changes in the natural environment, the level of technology and the various institutions of social interaction and control that maintain the culture (Willey 1953:1). On the basis of this assumption sociological inferences are drawn from the distributional data.

This approach has sought to generate reconstructions of settlement patterns through an understanding of site function. The particular form of settlement pattern reconstruction depends on the traits selected for observation. Southwestern Anthropological Research Group (SARG), for instance, reconstructs settlement patterns by identifying and correlating geographic locations with specific human activities over time (Euler and Gumerman 1978; Gumerman 1971). Inferences about past behaviors are based primarily on the proximity among present-day environmental features and archaeological site patterns (Sullivan and Schiffer 1978: 172). In other cases, settlement patterns are reconstructed by temporally and spatially

bounding cultural traits in order to interpret the changes in the distribution of the traits within an evolutionary framework. The traits may be items of material culture that reflect level of technology (Clark 1975; Martin and Plog 1973; Ritchie 1956), or site counts and settlement size that are equated with population density (Hole 1968; Sanders *et al.* 1979; Schwartz 1956; Sears 1956).

In the preceding cases behavioral interpretations again consist of identifying regularities in patterns. Since it is usually impossible to identify contemporaneous sites within a settlement system, the pattern definition is based on an equilibrium assumption. Thus, a settlement pattern represents a period of “stable” adaptation, and change is marked by the appearance of a new pattern that represents another period of stability. Patterns, then, are the normative behaviors that archaeologists spatially and temporally bound. Thus, the continuum of spatial and temporal processes is fragmented, creating artificial homogeneity and pattern (Wobst 1978).

The focus on normative patterning masks the dynamic qualities of settlement behaviors—the very social processes that we want to understand. Variation through time and space informs us about the dynamics of social systems that affect the spatial arrangements of settlements over a landscape. The variability in the configuration of settlements reflects both social and environmental stresses that vary over time and space. Stresses and responses to stresses occur along multiple axes, so that settlement patterns are the product of multiple axes of variability. In most cases, however, the pattern as constructed by the archaeologist tends to represent the mean or mode of one axis of variability, thereby ignoring other variations. In addition, pattern as a static description obscures the processes that structure the regional organization of settlements. Processes affecting the regional distribution of settlements occur along a variety of axes, as well as on a variety of scales. Therefore, in the absence of strong inference (Platt 1964), the reduction of variation to a pattern along a single axis of variability lends little insight into the dynamics of regional settlement systems.

### Subsistence-Settlement Studies

Subsistence-settlement studies are a genre of settlement pattern studies that include consideration of general ecology with a concern for human adaptation. The starting place is the resource base and its effect on subsistence strategies. They therefore affect settlement location, which in turn places various demands on the social structure. Given their interest in social behavior, archaeologists in the 1960s and early 1970s tended to combine a strong concern for man–land relationships with systems theory (Binford

1968; Flannery 1967; Struever 1968, 1971). Systems theory provided a framework for studying the interactions between the natural environment and components of the cultural system. Assuming that location, function, size, and distribution of settlements are direct products of these interactions, man-land relationships were viewed as explanatory mechanisms for culture change (Binford 1964:440). Proponents of the subsistence-settlement approach argued that it allowed them to study and to explain culture process. The region was identified as the unit of analysis, and the procedure involved generating and testing hypotheses that explain the relationship between the natural and cultural environments.

In practice, there is a discrepancy between the goals and the procedures for studying culture process. Subsistence-settlement studies tend to focus on only one dimension of human adaptations: adaptation to the natural environment. Reconstruction is based on detailed descriptions of the environment and the resource structure. Even though the framework is a systemic one, the environment is seen as playing a causal role in culture change. The results tend to be reconstructions of a succession of subsistence-settlement systems—culture histories (Brose 1970; Fitting 1969; Fitzhugh 1972; Rippeau 1977; Sanger 1975; Struever 1968, 1971; Winters 1969). Thus, sociocultural change is explained in terms of factors external to the cultural system. The alternative, that change is a product of internal organizational dynamics, remains intractable in this approach.

In subsistence-settlement studies, the principal focus has been on subsistence, that is, on how the resource structure affects subsistence practices and concomitantly social organization. One approach has been to generate subsistence rounds using modal figures for resources and then to define settlement types on the basis of association with specific resources (Clark 1975; Clarke 1976; David 1973; Mellars 1976; Smith 1975; Spiess 1979; Thomas 1973; Winters 1969). Another approach has been sophisticated modeling of resource scheduling and use. This approach analyzes the variability in availability, abundance, and predictability of resources over time and space. Thus, it can track changes in diet, foraging strategies, and locational choice (Earle 1980; Jochim 1976; Keene 1979; Limp 1978; Reidhead 1980; Root 1978).

However, in both of these approaches, subsistence tends to be modeled as a local process. As a local process it may not be commensurate with the behaviors of the regional population. It is inappropriate to assume that the seasonal rounds and site distributions generated solely on the basis of local information will yield a meaningful reconstruction of social process within a regional settlement system. Development and maintenance of subsistence-settlement systems involve a number of organizational processes that transcend the limits of local environmental variability (Johnson 1977; Paynter 1980).

Subsistence need not be modeled as a local process. As will be shown in the next section, subsistence is only one aspect of production; production involves food and nonfood items, as well as the organization of the social units of production. Man–land relationships are only a single dimension of a regional subsistence-settlement system, and are a local one at that. Man–man relationships, the interactions among human groups in their social matrix, at the local and supralocal levels are another dimension. It is by combining these two dimensions that we can examine the social processes that affect the relationship between social organization and the spatial order.

## Summary

Settlement studies have contributed significantly to archaeology, creating an interest in social behavior and identifying new classes of data that have the potential of giving insights on human behavior. The approaches discussed previously, however, have factored out process, be it social or otherwise. They fail to consider the arena of social interactions whether these are looked at within the settlement or whether they extend beyond the boundaries of the settlement, as well as the interaction among human groups under different social and ecological conditions. Instead they tend to bound behavior in time and space, they primarily focus on economic behaviors, and they develop explanations of change in terms of factors external to the social system. Although their ultimate goal is social process, the traditional approaches usually are applied in a vacuum as far as social relations are concerned, and depend on a number of nonprocessual assumptions. Settlement archaeology for Trigger (1967:149) 15 years ago, was “merely a new term for something archaeologists have been doing all along.” We might echo this criticism and within the theme of this volume suggest that settlement studies have become “normal” science. They are methods for doing archaeology, for collecting data without first specifying expectations about how the material correlates of behaviors will be expressed in the archaeological record.

## SOCIAL RELATIONS OF PRODUCTION: AN AXIS OF VARIABILITY

In this section the social relations of production are put forth as an alternative point of entry for studying the social processes that structure the configuration of settlements. The social relations of production, the means of production, and social reproduction are theoretical constructs that focus

on internal rather than external factors. These concepts are elaborated here. Thereafter, I will consider how social relations of production are manifested in egalitarian societies and the implications for the configuration of settlements.

Although Marx used these concepts in developing his theory of the capitalist social formation, the concepts are appropriate for all social formations, capitalist and precapitalist and have been applied fruitfully (e.g., Bender 1978; Friedman and Rowlands 1978; Godelier 1977; Paynter 1980; Tilley 1981). Social formations differ in the articulations between the means of production and social relations of production and in the appropriation of surplus. Although it is well beyond the aim of this chapter to do a Marxian analysis of egalitarian social formations, the concepts provide a materialist framework for examining social processes and their reflection in the spatial order.

In order to bridge social theory with the products of behavior that make up the archaeological record, information is introduced as an intrinsic aspect of social process, linking settlements, techno-environmental conditions and social relations within a cultural system. All systems must exchange not only energy and matter, but also information, in order to survive and persist (Flannery 1972). When we talk about the articulations among parts of cultural systems, it is implicit that information moves within and among the parts. The direction, frequency, and intensity of information flows structure both man-man and man-land interactions. Information, thus, may be seen as the common denominator of human interaction. Information exchange on a regional basis mediates social processes, such as subsistence-settlement arrangements.

The exchange of information involves both random and nonrandom interactions. Among these exchanges, the nonrandom interactions have a durable quality and produce structure (Van der Leeuw 1981). Material flows accompany information exchanges, and accumulate where information flows are concentrated, in loci called settlements. Therefore, settlement patterns can be defined as the spatial configuration of regional information flows.

## Theoretical Constructs

Production is a social process. "It is by labor that men satisfy their basic needs. . . . All these needs are satisfied socially, that is to say not by purely physiological activity, . . . which results from mutual relations established between the members of a human group" (Mandel (1968:24). The relations of production are the social basis for coordinating individual ac-

tivity in the labor process and determine “the specific use to be made of the means of production and the distribution of the total social labor time and product” (Friedman 1975:162). The social mechanisms that integrate the various elements of production are often hidden in the kinship and political–religious structures of a social system (Godelier 1977), affecting the form and content of the social organization. The arrangement and articulation of these social structures vary with the particular mode of production. The social relations of production articulate with the techno-environmental conditions of production (means of production). The techno-environmental conditions, in turn, place constraints and offer opportunities for extracting from nature the material conditions of existence.

Mode of production refers to the specific manner in which the elements of production, forces of labor and laborers, articulate in order to produce and reproduce material life. Marx argues that a mode of production must create the conditions for its own perpetuation, by ensuring continuity of the material conditions of social existence. “No society can go on producing, unless it constantly reconverts a part of its products into means of production. . . . The society can reproduce or maintain its wealth on the existing scale only by replacing the means of production which have been used up . . . with an equal quantity of new articles” (Marx 1867/1977:711). In addition, the configuration of social relations is perpetuated both by ensuring genetic reproduction and by allocating a portion of the production for the maintenance of the social structure. Thus, every society generates a surplus that is destined for the maintenance of social obligations: to ensure the conditions of reproduction of the producer and for the maintenance of nonproducers such as children and the aged.

Societies differ in the means of mobilizing and appropriating a surplus. In egalitarian societies, for example, rules of generalized reciprocity can function as mechanisms of social reproduction; objects are exchanged for the purpose of meeting and creating social obligations. These serve to maintain the producer and the producer’s family in a social and natural environment (Marx 1953/1964; Sahlins 1972). Thus, surplus production places institutionalized demands on an individual’s labor. Every social process of production is a process of reproduction (Marx 1867/1977).

Depending on our research interests we can talk about social reproduction for different social units, from the individual to the socioeconomic system as a whole. Archaeologically, the settlement is a particularly appropriate unit for investigation of the regional arrangement of social relations. However, in order to understand the articulation among the parts of a society, we must recognize that the single settlement is not a closed system. The relations of production are not limited to the organization of production, distribution, and consumption within a single settlement, since re-

production of that unit may depend on production occurring outside it (Friedman and Rowlands 1978).

### **Information and Social Reproduction**

The coordinating mechanisms that reproduce a population over time are an integral part of the economic basis of a society. In egalitarian societies, reciprocity, kinship, marriage alliances, ritual, and material flows may function to reproduce the material and social conditions of existence, as well as the social conditions needed to maintain equal access to critical resources. Processing, storing, and transmitting information are essential for the reproduction of the material and social conditions of a population. It is important to conceive of social reproduction as a dynamic concept and of societies as oscillating along a continuum of increasing and decreasing complexity. Information is a critical variable that mediates these dynamics, and the way information moves both reproduces a society and creates new conditions of existence.

Two assumptions about information are made in this chapter. First, it is assumed that information relates to effective coordination of activity. Thus, the arrangement of information flows and the information transfer costs necessary to coordinate and integrate a system affects the organizational form of a society (Johnson 1977, 1978). The second assumption is that institutionalized social relationships are the channels through which information moves (Van der Leeuw 1981). The way information moves influences the reproduction of the system of social relations. The social relations of production, and concomitantly the arrangement of information flows, are one axis of variability structuring the spatial configuration of settlements.

### **SOCIAL RELATIONS OF PRODUCTION IN EGALITARIAN SOCIETIES**

I have pointed out some of the implications of settlement approaches for the study of social process, noting that the traditional approaches focus on the means of production and ignore the dynamics of the social relations of production. I have suggested that information mediates the social relations of production, and one way to investigate these social processes is to consider the arrangement of information flows over a regional landscape. In the following sections, I apply this logic and examine egalitarian social relations of production that are manifested on the level of the settlement and that affect the functioning of the larger system of social relations. I then

consider some characteristics of the material correlates of these social relations and the implications for the spatial configurations of egalitarian societies.

### **Egalitarian Societies**

Egalitarian societies are defined as those societies in which the members have equal rights of access to the resources that sustain life (Fried 1967). Equal access means that no economic or political privileges are obtained by control of resources. This definition serves as a baseline to bring out variation around and deviation from the norm.

Traditionally, hunter–gatherer band societies have been considered the stereotype for egalitarian societies, as well as an evolutionary baseline against which evolutionary trends can be defined (Fried 1967; Sahlins and Service 1960; Service 1962). The recent literature on prehistoric hunter–gatherers, however, hints that foragers may not be an evolutionary baseline because some of them exhibit quite “complex” behaviors. There are sedentary hunter–gatherers practicing forms of resource management reminiscent of agriculturists found in environments characterized as marginal (Bettinger 1978; O’Connell 1975; Rick 1980), as well as in highly productive, temperate environments (Lourandos 1977, 1980). In other instances, hunter–gatherers mobilize production in certain social contexts, suggesting differences in prestige (King, 1978; Wright 1978). And finally, evidence indicates that our prototypic hunter–gatherers, the !Kung San, have employed a variety of adaptive strategies over time, including periods of farming and herding (Schrire 1980).

The recent hunter–gatherer literature invites us to reconsider the evolutionary continuum. We can look for oscillations along the continuum and can think of egalitarian societies as including a variety of organizational forms that move along a variety of pathways of increasing and decreasing social complexity. To bring out this variability, egalitarian societies in this chapter are not restricted to hunter–gatherer systems. Instead, they include all nonstratified societies, including ranked societies, and egalitarian subsystems of state societies. By placing egalitarian societies on a continuum, we can examine the dynamics of egalitarian social relations, the consequences for the organizational forms of these societies, and, hence, their spatial order.

### **Modeling Social Process**

One way to examine the processes that reproduce the conditions for egalitarianism is to consider information as a critical resource. Given the

definition of egalitarianism, it can be argued that equal access to information must be maintained. Therefore, hoarding of information must be avoided; and information must be shared as equally as possible. Hoarding is avoided through social relations that establish and maintain multiple channels through which information flows. The establishment and maintenance of multiple channels keeps information crisscrossing over the regional landscape. Yet the maintenance of this system of communication has costs; these costs vary with the social and ecological constraints on the continuous and nondirectional flow of information in any regional environment. The social relations of production maintain the conditions for egalitarianism by reproducing the flows of information through the region; these flows have profound implications for the analysis of settlement patterns.

### Fluid Social Groups

Multiple channels, in terms of institutionalized social relationships, spread information over the regional landscape. One social mechanism for maintaining multiple regional channels is fluid membership within and among social groups. The fluid composition of social groups is characteristic not just of mobile hunter-gatherers but also of agriculturists living in permanent villages (Brown 1978; Heider 1970; James 1949; Plog 1978). Fluidity not only equalizes access to important resources but also keeps information circulating through a diversity of channels based on kinship, marriage, and various forms of reciprocal relationships.

The *hxaro*, a system of delayed gift exchange among the !Kung San illustrates how social relations mediate environmental variability, differential distance to resources, and hence differential access to resources. Although Wiessner (1977) describes the *hxaro* as a way to minimize environmental risks, it also can be viewed as a means for spreading information and personnel over a regional landscape, and for reproducing egalitarian social relations.

"The *hxaro* is not a relationship between two people, but a chain of people in which a person feels somewhat indebted to others along the chain" (Wiessner 1977:xlili). *Hxaro* chains extend hundreds of kilometers, but knowledge of others on the chain usually does not extend beyond eight links. A high proportion of *hxaro* links are with a core group composed of several households, made up of people who have worked out a relationship of trust and reciprocity over a long period of time. In addition, partners are distributed regionally in order to cover a wide range of needs: access to nonlocal resources, insurance of access to food sources in other areas during periods of local scarcity, access to mates, and access to information. These

needs take priority over distance, and the intensity and frequency of interactions vary over time.

*Hxaro* relationships provide a social basis for the movement of groups over the landscape and for changes in group or camp membership. The composition of camps is highly fluid, even though core groups are associated with specific waterholes, and surrounding resources. The size, composition, and viability of a core group is affected by changing patterns of social relations reflecting conflict, death, marriage, and visiting. However, these centrifugal forces produce the conditions for *hxaro* exchanges and maintain a system of social obligations. Maintenance of *hxaro* relationships is facilitated through frequent movement of personnel, and at the same time enhances equal access to resources, including information.

It is the arrangement of social relations that gives structure to the !Kung social landscape, rather than repeated use of locations over time. According to Wiessner (1977:316) a region is not delineated by geographic variables, but rather by a set of camps "who have worked out a regular pattern of landuse between them in order to take advantage of the seasonal resources of each camp's area of landrights." The regular pattern of landuse reflects the strength and duration of social ties among members of the different camps. Therefore, while certain locations may have time depth and repeated occupation, there is not social continuity at any given location over time. The arrangements vary over time with the composition of a camp's members.

Clearly, traditional approaches to location, in terms of techno-environmental features, will not provide insights into the dynamics of social relations. The local context of settlements may provide information on potential foraging strategies, but little information on the regional social arrangements that produce sites and variability in the accumulations of material culture over time and space. In the case of the *hxaro*, creating a composite pattern of site locations over a region will mask the fluid social and spatial relationships that structure the landscape. Yellen and Harpending (1972) note that the spatially segregated clusters of sites within one ecological zone need not indicate the presence of discrete local groups consisting of different sets of individuals. Tracking the location, number, and density of sites over a region may tell us about the occurrence of specific activities but not about the articulation among the parts of a society, nor about the social processes that generate the regional configuration of settlements.

## Regional Aggregations

Large-scale, regularly scheduled aggregations of some number of groups are another social mechanism for maintaining multiple channels and

for ensuring the circulation of all information. Lee (1979:361) suggests that periods of aggregation followed by periods of dispersal are universal and basic to the hunter-gatherer adaptation, even in vastly different kinds of environments. I would argue that aggregation occurs in some form among all egalitarian societies as a mechanism for maintaining regional social relations and therefore, mediating regional information flows (e.g., Flannery 1972; Gross 1979; Lourandos 1977, 1980; Peebles and Kus 1977; Robbins 1980; Thomson 1949).

Lourandos (1977, 1980), drawing on ethnohistoric accounts and on the archaeological record, provides an interesting example of a regional aggregation-dispersal system among Australian hunter-gatherers in a temperate coastal plain. Interactions occur on different social scales that correspond to a ranking by size of sociopolitical units from the largest, the Western District, through the tribal area and the dialect area, to the smallest unit—the band. On the band level, there are regional interactions involving 800 to 1000 people, who engage in cooperative activities such as large scale hunting drives. Intertribal aggregations are associated with ceremonies during periods of resource abundance, taking place on “neutral” ground centrally located and close to boundaries among the heartlands of several tribes. The band and tribal aggregations regulate the distribution of the population in relation to resource availability. Patterns of reciprocal access to resources are arranged to crosscut cultural and political boundaries. These arrangements are necessary, whether during times of abundance or scarcity, if equitable distribution of resources is to be achieved (Lourandos 1977:217).

Intertribal aggregations also control competition for resources. Summer intertribal meetings held at the center of the Western District excluded coastal groups, and cut across an existing tribal area. Reciprocal ties were formed mainly among bands occupying different ecological zones, and, at the same time, populations in competition for the most favored area, the coast, were regulated.

In other instances, competition for resources involves access to information. Some groups are excluded from intertribal ceremonial aggregations because they do not have a predictable resource base to support large-scale gatherings. In order for those groups to compete and to interact with local groups, they must modify their production by regularizing resource yields through resource management. By intensifying their production, increasing labor costs and information processing, they can then afford to hold and to participate in regional ceremonies and to share in the information pool.

The coordination of social relations, rather than techno-environmental conditions appears to structure aggregation sites. Although the resource base contributes to the structure of aggregation sites, it is only one variable

and not a determining factor. Aggregations renew the social conditions necessary to maintain equal access to resources, as well as to reproduce the material conditions for the maintenance of the population. It appears that the arrangement of reciprocal ties among groups in different ecological zones mediates seasonal variation. From season to season and year to year these reciprocal relationships may change. In some cases, the implication is that information flows accompanying social relationships may parallel the resource structure and that population distributions may correspond to the resource structure. In other cases, information flows rearrange the population so that it does not correspond to the modal resource structure, in order to meet changing social and environmental conditions (Moore 1977; Moore and Root 1979). This challenges the commonly held assumption that we can predict settlement patterns by reconstructing techno-environmental conditions. Tracking the resource structure masks the potential variability and flexibility in social arrangements that can transcend the constraints of the techno-environment.

In traditional settlement studies it is a commonly held assumption that changes in the mode of subsistence lead to the evolution of new forms of social organization. In the Australian example, intensification of production, in the form of resource management, is fostered by social relations that reproduce the conditions for equal access to resources, including information. In this case, it appears that social organization is not structured by subsistence strategies. Rather, changes in technology and the mode of subsistence reflect reorganization of the existing productive forces, directed by changing social relations (Le Gros 1977; Tilley 1981). This example supports Bender's (1978) hint that the development of agriculture is more related to the intensification of social relations than to increased productivity.

### **Social Relations of Production and Information Flows**

The underpinning of the configurations of social relations is information. In both examples discussed previously, the social mechanisms that maintain multiple channels of information enhance the circulation of information and inhibit the formation of permanent social and spatial boundaries. Boundaries are formed by constraints on information flows. Flexible social and spatial boundaries are an important condition of egalitarian societies, and this condition is maintained in order to avoid hoarding of information.

In the case of the !Kung San, the fluid composition of social groups ensures the regional circulation of information as well as variability in the

information content. Through frequent movement of personnel, *hxaro* relationships are maintained, perpetuating the conditions for equal access to information. In the Australian example, aggregations coordinate regional information, and information moves with few constraints through the complex of social relations. The costs of sharing information are reflected not only in the fluidity of personnel but also in the reorganization of subsistence practices among some tribes. The different costs of processing information within the Western District suggest asymmetrical relationships among groups. These dynamics create the conditions for increased activity coordination or increased information processing in order to inhibit the formation of boundaries. In general, as the costs of avoiding accumulation of information increase, it becomes increasingly costly to maintain equal access to information.

### Summary

Within egalitarian societies there are centripetal and centrifugal forces in the movement of information. The dynamics and variability in the organization of egalitarian societies reflect the degree to which the social mechanisms effectively maintain multiple channels and inhibit hoarding of information. It is important to recognize, however, that a range of variation can be accommodated in egalitarian societies. The nature of the accommodation may also account for some of the variability in the organization of egalitarian societies. Egalitarian societies vary in their ability to tolerate asymmetrical access to critical resources. In some instances social mechanisms mediate the uneven flows and reproduce the conditions for egalitarianism. In other instances the constraints on information flows amplify and create the conditions for the accumulation of information and stratification. It is these issues that need to be the focus of further research.

### ARCHAEOLOGICAL IMPLICATIONS

It has been argued that traditional approaches to settlement studies focus on variables that do not measure social process. An alternative axis of variability, social relations of production, has been proposed in order to gain insights into the social processes affecting the arrangement and articulations of settlements within a social system. Information is an intrinsic aspect of social relations of production where processing, storing, and transmitting information is essential for the reproduction of social relations.

Material flows often accompany information exchange, and these produce structure in the archaeological record. Therefore, the social relations of production can be tracked in the archaeological record by examining the material expressions of information. Information is encoded in or on artifacts and this accounts for a large proportion of the variation in material culture (Wobst 1977).

Artifacts are scattered over a regional landscape and localized accumulations of material culture are defined as settlements. Localized accumulations of material culture are products of information exchange. Those accumulations are also the remains of extractive and/or manufacturing activities. Settlement data track the variability in localized accumulations of material culture; more specifically, the variability in the arrangement of information flows over a regional landscape. I suggest that the products of information flows can be isolated in the context of localized accumulations of material culture (settlements), and then, that the variability in the arrangement of information flows can be tracked over the region. I discuss this approach with respect to egalitarian societies, and consider attributes of portable information and nonportable information to generate some expectations for their distribution patterns.

Having argued that hoarding of information is avoided in egalitarian societies, I will now derive some expectations for the archaeological record. This is a first step toward delimiting the material expressions of egalitarian processes in the archaeological record. However, as discussed previously, societies tolerate a range of variability, and it is expected that different arrangements of information flows create variation in the spatial configurations of egalitarian settlements. In order to recognize and understand this variation, a baseline needs to be defined against which deviations can be measured. The arrangements of information flows can be qualitatively measured by differentiating the attributes of portable information and nonportable information, as well as their spatial patterns over a regional landscape. As will be argued, portable information tends to enhance centrifugal flows of information, whereas, nonportable information tends to encourage centripetal flows of information. Those, in turn, affect the costs of maintaining the conditions for egalitarianism.

### Portable Information

Portable information refers to the nonextractive functions encoded on transportable artifacts (Binford 1962). Portable information transcends the limits of face–face interactions and broadens the number of individuals who

may have access to the information. Codes compact bits of information and thus, portable information can remind people about relationships and/or obligations without face–face interactions.

In egalitarian societies information must be eventually passed on and this is facilitated by information encoded on transportable objects. In order to ensure the circulation of information through multiple channels, it is expected that there is considerable repetition, as well as variety in the information exchanged. In egalitarian societies the labor invested in encoding information on artifacts reflects a premium placed on the sharing of information, so that the message and its source are frequently unimportant. This suggests qualitative differences in the characteristics of portable information in stratified societies, where messages are encoded to elicit specific responses on the part of the receivers. One way to ensure accessibility of information and inhibit hoarding is to make items that contain information highly visible. Therefore, items that are rare, elaborately designed, morphologically or stylistically distinct, and/or pervasive can all have high visibility in the context of other items of material culture (Wobst 1977).

Artifacts that are highly visible are expected to be curated, so as to perpetuate the conditions for egalitarianism, namely to keep information in circulation. Curation refers to artifacts that are conserved, exchanged and used by different individuals over the lifetime of the artifact. Curated items are not expected in burials nor caches, because both take information out of circulation and imply hoarding. These curated items are expected to have a low occurrence in localized accumulations, as well as a low correspondence to other artifact types within and among settlements. In contrast, noncurated items, items that do not articulate with the social relations of production, are expected to account for a high proportion of material culture in localized accumulations. Finally, curated items are expected to exhibit a high amount of variability, reflecting multiple exchanges and successive modifications. Traditionally, artifacts with a low frequency of occurrence and low association with other tool types in a “tool kit” are perceived as residuals, contributing little to the functional variability of settlements. However, if we approach the variation in settlements in terms of the attributes of portable information, we are likely to end up with different descriptions and interpretations of the variation, ones that I would argue are indeed sensitive to social process.

The regional distribution of portable information is expected to be relatively homogeneous among settlements if hoarding of information is avoided. The distribution of miniature engravings in southwestern France during the Magdalenian IV period illustrates this expectation. These engravings are found in cave sites in the Pyrene foothills and Dordogne valleys. They fit the criteria of portable information, visible objects, in that they are

rare as well as elaborately designed. According to Sieveking (1976), the engravings show a striking similarity in style and subject. With southwestern France as the unit of analysis, intraregional distribution patterns are not encountered for the engravings, implying curation and multiple channels of communication. "The pattern or lack of it, suggests that there must have been great flexibility of social grouping in Southwest France at this period" (Sieveking 1976:594). It seems reasonable to interpret this lack of patterning as evidence for social arrangements that avoid hoarding of information. By the Late Magdalenian, localized distribution patterns of some engravings imply changes in the social arrangements and, hence, information flows (Sieveking 1976).

Variability in the organization of the social relations of production affects the degree to which information is socially and/or spatially bounded. As the costs of maintaining equal access to information increase, it is expected that there should be evidence for constraints on information flows, increasing the directionality of information flows. Constraints on the movement of information may be expressed by concentrations of portable information, thereby creating competition for participation in the information pool. These processes may be manifested by burials that contain elaborate and/or large quantities of burial goods, such as Sungir where over 3000 beads were placed in a group burial (Klein 1980). Under these social conditions, settlements are expected to contain an increased number of items that can be defined as portable information.

### Nonportable Information

Nonportable information refers to features of the landscape that have been transformed from a random to a nonrandom condition, in order to mark unambiguously social and/or geographic boundaries. Permanent houses, storage pits, buffer zones, cave paintings, or cemeteries are examples of nonportable information. Accumulations of information constrain the movement of information and create boundaries. Under some conditions, short-term accumulation of information can be accommodated through social mechanisms that create centrifugal forces in the flows of information. Long-term accumulation of information violates the conditions for egalitarianism. The degree to which individuals or groups are socially bounded and spatially circumscribed has implications for the occurrence of nonportable information over a regional landscape, as well as the variability in the organizational form of egalitarian societies.

Since flexible social and spatial boundaries inhibit hoarding of information, under most conditions, nonportable information is not an expected component of egalitarian settlements. Classes of material culture that can be

categorized as nonportable information are expected to contain little information, such as !Kung bushman huts. However, in some instances, nonportable information may be a feature of aggregation sites. Aggregations mediate temporary hoarding of information, and demarcation of these sites may enhance the conditions for sharing all information. Johnson (1977, 1978) has shown that there is a ceiling on the number of interactions given horizontal integration of information. Therefore, aggregation sites that are predictable in time and space may be necessary for effective coordination of activity, and to ensure the circulation of all information. In order to guarantee the regional integration of information flows, specific activities such as initiation and ceremonial exchanges may occur only at these sites. These activities need not involve attempts to control information flows. Ignoring postdepositional processes, a high density and diversity of curated items are expected at these sites in the context of the regional array of localized accumulations. However, caches or concentrations of surplus labor are not expected, if hoarding of information is avoided.

The conditions for egalitarianism may be threatened as a system grows, creating centripetal forces in the movement of information. As systems grow, increasing the number of activities and hence the amount of information processed, there is an increased premium on regularizing personnel and information channels in order to coordinate effectively the system (Flannery 1972; Rappaport 1971). These centripetal forces create the conditions for social arrangements that regularize interactions with the social and natural environments, and hence increasingly bound individuals socially and spatially to a locality. On the level of the settlement we can expect a high number of material expressions that correspond to increased conformity of behavior and spatial replication of personnel. Under these conditions an increased number of nonportable items are expected to be encoded with information, for example, the houses at Mezhirich (Clark 1977), and they are expected to be a recurrent feature of the regional landscape. Tilley (1981) implies that the construction of megalith tombs during the Early Neolithic in northwestern Europe was an ideological means of counteracting the centripetal movements of information flows created by social relations stressing social boundedness. However, the coordination and costs of the social labor involved in these constructions must have placed constraints on the fluidity of group membership and hence, information flows.

## CONCLUSIONS

Settlement pattern studies do have potential for providing insights into social process. However, I have tried to suggest that the point of entry is

crucial in the approach to the study of culture process. My point of departure is a concern for the processes that reproduce the conditions for egalitarianism, and how these processes are reflected in the spatial configuration of settlements. When responding to questions of structural changes that increase cultural complexity and lead to stratified centralized forms of organization, a prerequisite is an understanding of the nature of egalitarianism, the constraints on egalitarian societies, and the way in which these constraints can amplify and transform the arrangement of resource flows.

In order to delimit one end of the continuum of egalitarian societies, I have put forth total information sharing as a condition that maintains egalitarian social relations over a regional landscape. Clearly, the next step is to consider deviations from this norm, and the consequences for egalitarian social relations. Ecological, social, and demographic conditions cause centripetal and centrifugal movements in the flows of information, creating the conditions for differential access to information. Future investigations will require consideration of how information is channeled and how directional flows are mediated, in order to avoid socially regulated unequal access to information and other strategic resources. It is these dynamics that account for much of the variability in the organizational forms of egalitarian societies, and as discussed previously, they are tractable through material culture. The potential, then, of settlement studies depends on the questions we pose about the dynamic relationships between the forces of production and the social relations of production.

## ACKNOWLEDGMENTS

I want to thank H. Martin Wobst who freely shared and nurtured ideas, making significant contributions to the final version, and Joan M. Gero who was instrumental in helping me streamline my thinking. Inspiration for the questions posed regarding egalitarianism evolved from questions posed by Robert W. Paynter in his dissertation and forthcoming book. The chapter benefitted from comments on an earlier draft made by Barbara Bender, Gregory Johnson, and Dean Saitta. I appreciate the contributions of all these people, but I alone must take responsibility for the final product.

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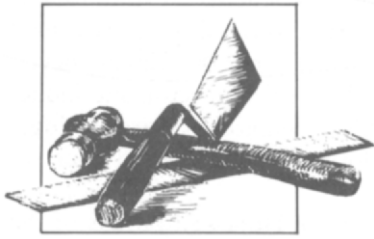
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# 10

## The Ecological Perspective in Highland Mesoamerican Archaeology

RICHARD E. BLANTON

### INTRODUCTION

The growth of the early civilizations represents one of anthropological archaeology's most challenging but unmanageable problems. It is unmanageable because the complexity and variety can overwhelm. Given this, there are basically two strategies that researchers can adopt. One is to ignore the complexity and try to find one factor or only a few that cut right to the core of things and offer a maximum of explanatory power with a minimum of bother. The other is to try to match the complexity of the reality with complexity and richness in theory and method. The latter probably dictates that the researcher be grounded in ethnology, demography, economics, political organization, home economics, human biology, ecology, and probably several other disciplines as well. Both strategies have advantages and disadvantages. At this moment in Highland Mesoamerican archaeology a debate is going on concerning the relative merits of the two

strategies. This debate has been prodded into animation by the fact that during the past 20 years, research in this part of the world turned so decidedly in the direction of the former approach—that favoring simple explanations for complex problems. But challenges to this accepted wisdom are being raised. As Eric Wolf put it, in commenting on the contemporary “cultural ecology” of Highland Mesoamerican archaeology:

Certainly the simplicity and directness of the model are commendable, especially when one is interested in global relations and global trends. It would seem, however, that when interest turns to an analysis of the critical turning points in the spiral which connects population → technology → societal differentiation → controls, more complex models will be required [1976:7].

That Wolf should criticize cultural ecology is especially understandable because he was one of a group of scholars—including Pedro Armillas and Angel Palerm among others—who had labored during the 1950s to add new dimensions in theoretical complexity and anthropological sophistication to the study of early mesoamerican civilization. Their concerns, and the literature they used were, by and large, new to mesoamericanists, and highly stimulating: they drew from the ideas of Julian Steward and Karl Wittfogel concerning the role of environment and subsistence strategies in cultural change, from Wirth and Redfield concerning urban versus rural dynamics, and from geopolitical analysts’ ideas about the nature of core-periphery relationships to mention only a few sources (cf. Palerm 1955; Palerm and Wolf 1954–55, 1957; Wolf 1976). An important byproduct of the new intellectual impetus came about through the realization that the traditional site-oriented archaeological methods used in Mesoamerica were largely inappropriate in the light of the new questions being asked. However, the method of intensive surface survey of settlements over meaningfully large areas—that had been pioneered by Willey (1953)—seemed to be one way out of the methodological dilemma. In 1959 a conference was convened in which it was decided to begin such studies in the Teotihuacán Valley, a part of the Valley of Mexico that housed Teotihuacán, Mesoamerica’s largest city of the Classic period. Settlement pattern survey proved to be such a feasible and productive method that subsequent to the Teotihuacán Valley study a major commitment was made in effort and funds for similar work in other parts of the Valley of Mexico. Now nearly the entire Valley of Mexico has been completed, a survey project incomparable in scope to any other world region (Armillas 1971; Blanton 1972; Millon 1973; Millon *et al.* 1973; Parsons 1971; Sanders *et al.* 1979). And this method was implemented in other of the high, semiarid mesoamerican valleys, notably the Valley of Oaxaca, building a large comparative base for studies on the

growth of cultural complexity. (Blanton 1978; Blanton *et al.* 1979, 1982; for an overview and comparison of the survey projects, see Blanton *et al.* 1981).

It is unfortunate, however, that the survey data have become the most important relics of the intellectual efforts of Wolf, Palerm, Armillas and the others who had brought about the new problem orientation. I say this because from a theoretical perspective their efforts have yet to bear fruit. Since the 1960s, the complexity, eclecticism, balance, and anthropological sophistication characteristic of the thinking of this distinguished group have given way to the simplicity, reductionism, and unilineal determinism of many contemporary cultural ecologists. I describe their theory next.

## POPULATION DETERMINISM

In the preceding section I used the phrase cultural ecology in reference to the paradigm that is dominant in the anthropological archaeology of Highland Mesoamerica today. This usage could be misleading, however, since there are other brands of cultural ecology practiced within anthropology (Flannery 1977; Rappaport 1971; Vayda and Rappaport 1968). To avoid confusion, I will refer to the version of cultural ecology under discussion here as “population determinism,” a title that reflects the overriding importance that is placed on population growth as the prime mover of cultural evolution.

Population determinism was developed as a means for dealing practically with the chaotic complexity that faces the researcher interested in the growth of civilization. Progress in explanation, it is argued, requires cutting away at variety and nuance until a few key features or a single key feature can be found with great illuminating power. And thus arose the agreement that for studying the growth of civilization this key feature would be population growth, leading to pressure on resources. A model was then constructed in which virtually every important aspect associated with a civilization's growth—an increasingly complex division of labor, agricultural intensification, political centralization, and so on—could ultimately be traced to the consequences of population growth (and episodes of civilization decline, as well as of growth, were attributed to population pressure). Population growth (which is assumed to be omnipresent) leads eventually to pressure on agricultural resources, which in turn results in conflict. Locally, political institutions develop to quell these disputes. Regionally, groups with a “demographic advantage” and a “more productive agricultural support base” eventually dominate weaker groups, establishing polities of regional or macroregional scale in the context of a militaristic environment brought

about by population pressure on the resource base (cf. Logan and Sanders 1976; Sanders *et al.* 1979; Santley 1980).

Of interest to me here are three implications of the general theory:

1. Population growth and population pressure should precede periods of disequilibrium, warfare, and change.

2. Since evolutionary "success" is environmentally determined, dominant centers should be located adjacent to optimal agricultural land. In other words, there should be a good fit between population and resource distribution.

3. Since the theory has general applicability, the evolutionary sequences of different regions should be highly similar, differing only in minor features such as the amount of time required for the evolutionary processes to be played out (Sanders and Webster 1978).

As Wolf pointed out, the theory is commendable for its simplicity and directness. It has another advantage. The critical variables, population growth and the agricultural potential of regions, are just the kinds of variables for which data can be collected in the course of the settlement pattern surveys. It was as these data started to come in, however, that we began to realize that population determinism is just another archaeological "hammer." Too much had been asked of a simplistic scenario of change. It could not meet the challenge.

## THE DISCOVERY OF ANOMALIES

Even before the data were available to evaluate the population determinists' theory, some criticized it on the grounds that it ignored certain things not ignorable in understanding the growth of early civilizations, namely, the role of religion and ritual (Flannery 1977; Millon 1973). Others argued on theoretical grounds that population growth cannot legitimately be assumed to be a given. Population growth, too, requires explanation (Blanton 1975; Cowgill 1975). But the settlement pattern data are providing the most compelling evidence for anomalies in the theory:

1. In both the Valleys of Oaxaca and Mexico, two of Mesoamerica's most important nuclear regions, there was a tendency for the human populations to have underused agricultural resources. Even during key periods of disequilibrium and change, there was no indication of pressure on resources, and major episodes of population growth tended to follow important periods of change rather than preceding periods characterized by

change (Blanton 1981; Blanton *et al.* 1979, 1981:1982. Kowalewski 1980, 1982; Sanders 1976; Sanders *et al.* 1979).

2. In the Valley of Oaxaca, where detailed studies of the distribution of agricultural resources have been done, the expected close fit between the distribution of prime land and dominant centers was not found (Blanton 1978; Kirkby 1973; Kowalewski 1980, 1982). Monte Albán, for example, which for centuries was the region's major political center, was located in one of the valley's more marginal sectors from the point of view of agricultural production (Blanton 1978; Kirkby 1973; Kowalewski 1980, 1982); and

3. It has become increasingly clear that the evolutionary histories of the Valleys of Oaxaca and Mexico were distinct in a number of ways not explicable in terms of population determinism. There were differences in terms of the organization of production and distribution, in forms of regional political organization, and in the forms and functions of the respective political capitals (Blanton 1978, 1980, 1981; Blanton *et al.* 1981; Marcus 1980).

The discovery of these anomalies proves neither that human populations never exceed their environmental limits, nor that population pressure never has a causal role in cultural evolution. But as a general theory of cultural change, population determinism obviously has severe limitations. The evidence suggests the need for new approaches.

## NEW APPROACHES

If nothing else, our experience with population determinism has taught us to maintain a healthy skepticism about unicausal "prime mover" theorizing. And yet we have no clear indication as to the nature of superior alternatives. We might be able to learn something, however, from the physicist "bootstrappers," who themselves have recently expressed pessimism concerning the likelihood that there will ever be a unified theory of matter. As described by Capra (1975:285), the bootstrappers argue that:

the phenomena [in particle physics] are so complex that it is by no means certain whether the complete self-consistent [framework] will ever be constructed, but one can envisage a series of partially successful models of smaller scope. Each of these models would be intended to cover only a part of [particle] physics and would therefore contain some unexplained parameters representing its limitations, but the parameters of one model may be explained by another. Thus more and more . . . phenomena may gradually be covered with ever-increasing ac-

curacy by a mosaic of interlocking models whose net number of unexplained parameters will keep decreasing. The adjective "bootstrap" is thus never appropriate for any individual model, but can be applied only to a combination of mutually consistent models, none of which is any more fundamental than the others. As Chew put it, "a physicist who is able to view any number of partially successful models without favoritism is automatically a bootstrapper."

I am not now in a position to specify which set of models might be substituted for population determinism to explain the growth of prehispanic civilization in Mesoamerica. The development of a satisfactory set of such interlocked models will be decades in the making. But I can give some indication of how such an approach might proceed, by briefly describing a theory that I and my colleagues, Steve Kowalewski, Gary Feinman, and Jill Appel, put forth to explain the origins of chiefly-level political control during the Early Formative of the Valley of Oaxaca. Although untested, I hope this set of propositions will serve as an example of the kinds of evolutionary processes that can occur in the absence of population pressure or competition for resources (a fuller discussion, along with our Early Formative data, can be found in Blanton *et al.* 1981; Fisch 1982). The theory can be expressed as a set of related propositions as follows:

1. As early as the Early Formative San José phase (ca. 1200–900 B.C.) there is ample evidence in the Valley of Oaxaca for the kind of centralization of power and resources associated with the growth of a chiefdom (Flannery 1968, 1976; Flannery *et al.* 1981). Our settlement pattern surveys indicate a relatively small human population occupying the valley at that time, at nowhere near the potential carrying capacity of the area (Fisch 1982). The population aggregates were not "too large" vis-à-vis food resources.

2. On the contrary, it struck us, given the very small size of most Early Formative population aggregates, that a key problem would have been that populations were too small to assure long-term viability as demographic isolates. Computer simulations of small human populations indicate that the probability of extinction due to disproportionate sex-ratio values is higher for small populations than for large populations (Ammerman 1975; Wobst 1974, 1975). Only one community, the head town of San José Mogote, was probably large enough to avoid the possibility of serious sex-ratio imbalances. But even it had only about 500–700 persons. Other populations would by necessity have participated in interpopulation exchanges of personnel to even out imbalances. And populations in the mountainous rural periphery surrounding the Valley of Oaxaca were probably even smaller than the environmentally better-situated valley groups. We expect that these marginal populations would have looked to the somewhat larger,

more stable populations of the Valley of Oaxaca, in particular that at San José Mogote, as an especially dependable source of marriage partners in stressful times. Thus, in a circulating connubia extending over much or all of the Southern Highlands macroregion, the Valley of Oaxaca would have had a special role to play, acting more frequently as a donor of people in a donor–recipient system.

3. Since it has often been noted by ethnographers that wife-givers accrue prestige at the expense of wife-receivers, a relationship is established whereby there would have been, over time, a net accumulation of prestige by valley populations (cf. the similar scenario proposed by Friedman 1975; Friedman and Rowlands 1978). This relationship also establishes a relationship of accrual of wealth by groups with higher prestige, since they can demand higher bride-price payments (Friedman 1975; Friedman and Rowlands 1978). A positive feedback process is thus established, if, as is usually observed, the accumulated wealth is ceremonially redistributed or used for presentations that can further increase the prestige of the donor group.

4. Prestigious, relatively wealthy groups attract followers whose presence bolsters the size of their population. This further reduces their risk of demographic fluctuation, while increasing their productive capacity and military potential. This kind of migration at the same time reduces the demographic and military viability of marginal populations. The marginal populations thus become even more dependent on the central group for marriage partners, and perhaps military protection, while transferring more and more of their surplus production to the same dominant group, an additional positive feedback loop.

5. Since demographic success, wealth, and prestige are thought to emanate from supernatural sources, it comes to be believed that the central group has exceptionally close ties to the supernatural. The dominant group is at that point able to intervene itself between the supernatural and others, and collects tribute in exchange for these intermediation tasks.

Although completely untested, I think that this set of propositions represents a more satisfactory means for approaching the problems we face in understanding the beginnings of centralized power in the Valley of Oaxaca than is provided by the traditional cultural ecological model. It is based on an important fact of life for Early Formative people: most population aggregates were too small for long-term demographic viability, and were thus forced to participate in an exchange system. A chiefdom grew out of the fact that over time these exchanges became unequal exchanges due to unequal demographic risk.

Models such as this will require that we go further afield for data collection than our regional surveys have taken us. Up to now, guided by the

population determinists' model, we have assumed that to understand the growth of civilization we should study the regions where the most important manifestations of that change occurred, the core areas like the Valleys of Oaxaca and Mexico. But is it possible that changes in the direction of increased power and wealth in the cores occur only as a concomitant of decreased power and wealth in the peripheries? This is precisely the argument made by a group of economists, the *dependency theorists*, in their explanations of global patterns of development—underdevelopment (Frank 1969; Wallerstein 1974; cf. Foster-Carter 1976).

## CONCLUSION

I am not proposing that this scenario of change in Early Formative Oaxaca be adopted as a new “hammer” to replace the population determinists' *hammer*. It could not possibly have the generality claimed by the proponents of population determinism. First, it is a scenario that is pertinent only to the period of the earliest growth of chiefdoms in Oaxaca. In later periods, local populations were larger (although not necessarily large enough to stress their environmental limits), and were therefore probably demographically viable even in the absence of interpopulational exchanges of personnel. Other models will have to be developed to explain changes that occurred later in the sequence when states, market systems, and urban centers replaced the chiefdoms of the Early Formative. Second, this scenario was developed with the particular features of the Southern Highlands macroregion in mind. It visualizes a large, broad Valley of Oaxaca surrounded by the rugged mountainous highlands of the periphery with small, marginal populations. This kind of setting may not be present in other parts of Mesoamerica or elsewhere where chiefdoms developed. For example, the central Highlands of Mexico, the setting of the Valley of Mexico, is topographically quite distinct. The theory is thus restricted both chronologically and geographically. It is, to paraphrase Capra, a partially successful model of small scope. As such it will not be satisfying to the population determinists or to proponents of other prime mover theories. But it is probably a more reasonable way to begin resolving a set of problems that evidently are not approachable through simplified, unicausal solutions.

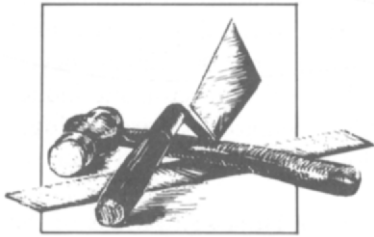
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# 11

## Expanding the Scope of Settlement Analysis

ROBERT W. PAYNTER

Central-place models and rank-size analysis are important tools for interpreting stratified society settlement patterns. Numerous reviews and compendia consider the use of central-place models in archaeology and anthropology (e.g., Blanton 1976a; Chang 1968, 1972; Clarke 1977; Earle and Ericson 1977; Hodder 1978; Hodder and Orton 1976; Johnson 1977; Renfrew and Cooke 1979; Skinner 1964, 1965a,b, 1977; B. D. Smith 1978; C. A. Smith 1976a). Rank-size analysis has achieved particular popularity as one bridging argument from central-place models to archaeological data (e.g., Blanton 1976b; Crumley 1976; Johnson 1977, 1980a,b; Kowalewski 1980; Paynter 1982). However, as is true with any model or analytical technique, there are limits to the usefulness of these ideas.

Recalling Flannery's (1976:162) distinction between settlement systems and patterns provides a framework for investigating limits of central-place models and rank-size analysis. As Flannery points out, settlement systems are the explanatory models used to interpret empirical patterns; patterns, on the other hand, are drawn from the settlement data. In the

following, I consider three problems in using central-place systems to interpret archaeologically recovered settlement patterns.

First, due to uncontrollable biasing processes, some sites of a past system will have been eliminated. Thus, archaeologists are not likely to recover the entire pattern of the past system. Second, due to the spatial scale of past ways of life, any settlement pattern is not likely to be interpretable with one complete settlement system. Third, the institutional assumptions underlying conventional central-place systems are not likely to be relevant for interpreting past sociocultural systems. Obviously, interpreting incomplete patterns with models of entire systems, or using inappropriate behavioral assumptions leads to misleading results.

Whereas these points suggest that central-place systems cannot be simply applied to settlement patterns, they do not, as I argue subsequently, lead to the rejection of this approach. Rather, they stand as challenges to develop and refine available methods and theories. Thus, in more fully discussing these problems, I also make some suggestions for the circumvention of these problems. In this regard, rank–size analysis is particularly useful in illustrating alternative methodological strategies.

This chapter is organized into two sections. The first presents the role of central-place models and rank–size analysis in analyzing settlement patterns. The second section explores the three problems and suggests some concrete methodological strategies and theoretical directions to expand the usefulness of central-place models and rank–size analysis.

## SETTLEMENT SYSTEMS AND THE RANK–SIZE RULE

### The Analysis of Hierarchies with Central-Place Models

As in any field of scientific research, theory building in settlement studies follows two basic tendencies: (1) generalizing from patterns and (2) deductions about abstract systems. The generalizing approach is concerned with describing the range of settlement patterns encountered in the archaeological record. These include monographs and articles describing relatively egalitarian systems (e.g., Binford 1978, 1979a,b; Thomas 1973) and relatively stratified systems (e.g., Adams 1965, 1981; Adams and Nissen 1972; Blanton *et al.* 1979; Johnson 1973; MacNeish 1972; Parsons 1971; Sanders *et al.* 1979; Willey 1949, 1956). The payoffs of this approach include general temporal and spatial typologies of empirical settlement patterns (e.g., Chang 1972) and general correlations between settlement pattern types and other dimensions of culture (e.g., Beardsley *et al.* 1956). Thus, the generalizing approach provides intersubjective categories for describing spe-

cific settlement histories and provocative data concerning ethnological theory (e.g., R. McC. Adams 1975; Johnson 1973; Sanders *et al.* 1979; Willey 1979; Wright and Johnson 1975).

A second broad approach elucidates the abstract relationship between settlement patterns and dimensions of a cultural system. A frequently studied relationship, inspired by general ecology (e.g., MacArthur 1972; Pielou 1969), is between the subsistence practices of a group and the resultant settlement location(s) (e.g., Binford 1964; Flannery 1968; Green 1980; Gummerman 1971; Jochim 1976; Keene 1981; Perlman 1976; Wilmsen 1973; Zimmerman 1977). However, not all research has concentrated on human–environment interactions. For instance, demographic processes, particularly growth processes, have been linked to such settlement phenomena as waves of advance (Ammerman and Cavalli-Sforza 1979; Mosimann and Martin 1975), centralizing tendencies (e.g., Wobst 1974), and pattern evolution (e.g., Swedlund 1975). The frictional effects of space on interactions have been rigorously studied with distance decay models (e.g., Hodder and Orton 1976:98–154; Renfrew 1975). The effects of varying political economic institutions have been studied in prehistoric (e.g., Blanton *et al.* 1979; Johnson 1980b) and historical settings (e.g., Ceci 1977, 1980; Kramer 1979; Lewis 1977; Paynter 1982; C. A. Smith 1978). And, recent work considers how ideology (e.g., Marcus 1973), the ideology of legitimization (Kus 1981), and the nonuniform flow of information (Moore 1981) all constrain settlement patterns.

A key characteristic of stratified society landscapes, found in studies of patterns and systems, is that settlements are arranged in hierarchies (e.g., Flannery 1976) with a large number of small places and a small number of large places. Empirical studies have greatly refined the site typologies used to analyze hierarchies, as can be noted by comparing Willey's (1949:7) pioneering 4 site typology to the 15 types used by Sanders *et al.* (1979: 55–58) to study the Basin of Mexico. In theoretical studies, the major interpretive models are based on central place theory (e.g., Blanton 1976a; Crumley 1976; Evans 1980; Johnson 1972, 1975, 1977; Paynter 1982; Smith 1979; Steponaitis 1978).

Central-place theory includes approaches to a variety of problems concerning the variation in settlement size and location (e.g., Berry 1967; Christaller 1966; Lösch 1967). Reviews of these approaches are readily available in the anthropological and archaeological literature (e.g., Blanton 1976a; Crumley 1976; Johnson 1977; Smith 1976a; Skinner 1964, 1965a,b). Rather than cover this ground again, I briefly consider a few essential concepts used in studying settlement hierarchies.

Two key concepts used to model and interpret hierarchies are the economic functions of a place and the frictional effects of distance. Many

versions of central place theory conceptualize settlements as aggregates of economic functions. The economic functions often involve exchange, retail exchanges in Christaller's models (1966) and wholesale exchanges in Vance's (1970) approach. Hierarchies, in theory, result from different places supporting different numbers and kinds of economic functions.

Algorithms for allocating economic functions to places are usually based on the frictional effects of distance. All economic exchanges involve moving goods and/or people over space. Overcoming the friction of space (i.e., movement) involves someone's expenditure of energy. Furthermore, central-place theory suggests that people will not travel equal distances to engage in all exchanges. Some exchanges attract people from great distances, whereas others only attract people from nearby. The maximum distance people move to enter an exchange is referred to as the range of the function (e.g., Smith 1976b:12). Thus, a settlement hierarchy has different-sized settlements supporting different numbers and kinds of exchanges that involve people from different-sized surrounding areas.

One of the major versions of central-place theory, Christaller's (1966), develops hierarchies as follows. This approach assumes that if a place offers a large-ranged function, then it also offers all smaller-ranged functions. The allocation of ranges to places is based on various assumptions regarding the extent to which ranges of items from different places overlap. For instance, the marketing principle allocates large- and small-ranged functions so that any smaller place is equidistant from three larger places. This arrangement, known as  $k = 3$  lattice, maximizes range overlap and minimizes the spatial costs of interaction between people of large and small places. The transport principle allocates economic functions so that any smaller place is equidistant to two larger places. This arrangement, known as  $k = 4$  lattice, minimizes spatial costs for interaction between relatively large places. The administrative principle  $k = 7$  lattice arranges economic functions to minimize range overlap, so that smaller places are closest to only one large place (e.g., Christaller 1966:71–80; Smith 1976b:18–23).

Central place models in general, and the Christaller approach in particular, have a number of useful characteristics for interpreting archaeologically derived settlement patterns. For instance, there is an intuitive connection between the number of economic functions supported at a place and the physical size of the place (a point subsequently given more attention). Thus, central-place models provide a behavioral correlate for archaeological data, namely that different-sized settlements supported different numbers of economic functions.

A second useful point involves the range of an economic function with the concept of the friction of distance. The Christaller assumption that large-ranged places support all smaller-ranged functions means that large-

ranged sites are also the sites supporting the greater number of functions. This matches the intuition that large places tend to influence large areas, whereas smaller places tend to influence smaller areas. This can be put to use in studying the degree of internal differentiation of a sociocultural system. For instance, the idea that some sites had more influence than others implies a differentiation regarding the exercise of power. Wright and Johnson (1975) suggest that settlement hierarchies reflect the degree of differentiation, and are especially useful for measuring the emergence of the control functions associated with the evolution of the state.

Finally, the variety in settlement hierarchies based on the different assumptions of range overlap and spatial-cost allocation suggest the operation of different kinds of sociocultural systems. Most well known is Skinner's identification, in China, of transport principle hierarchies in mountainous areas (1964), market principle hierarchies on alluvial plains (1964), and the problems associated with some socialist strategies based on administrative hierarchies (1965b). In an archaeological application, these different settlement hierarchies have been used to argue for the operation of market economies versus administered economies in the Valley of Mexico (e.g., Smith 1979; Evans 1980).

The Christaller approach by no means exhausts the variety of hierarchy models. For instance, Lösch (1967) assumes a society of "rational men" and allocates economic functions to places based on the marginal utility associated with the specific location. Isard (1956), using similar behavioral assumptions, incorporates production along with exchange functions in developing model landscapes. And, in yet another approach, Vance (1970) uses a behavioral model dominated by wholesaling activities, rather than the retail activities associated with Christaller.

However, all these approaches share two characteristics. First, settlement hierarchies emerge from different numbers and kinds of economic functions occurring at different places. Second, the allocation of functions to places reflects the differential spatial costs associated with the functions. Even though the specific behavioral assumptions used to generate these models will subsequently be criticized, associating settlements with economic functions and allocating functions on the basis of the costs of overcoming the friction of distance, provide two useful assumptions underlying any settlement study.

### Rank-Size Analysis

Substantial methodological problems beset interpretation of settlement patterns with central-place models. For instance, present methods of fitting

two-dimensional, central-place model landscapes have yet, in archaeology, to develop beyond eyeballing model and empirical maps (e.g., Johnson 1972; Paynter 1980:311–362; Smith 1979). The more rigorous procedures evaluate single dimension characteristics of the model, such as the nature of the regional hierarchy (e.g., Johnson 1975).

Hierarchy analysis, however, presents problems for most archaeological survey data. For instance, multivariate techniques are frequently used to identify settlement hierarchies with contemporary data (e.g., Berry 1967: 26–40; Berry and Kasarda 1977:305–335). Archaeological survey data rarely include more variables per period than site location and site size. Thus, isolating discrete hierarchies to compare with Christaller's central-place models is a problem.

A solution lies in analyzing the entire distribution of settlement sizes without requiring the isolation of individual hierarchical levels. Rank-size analysis is one such analytic procedure (e.g., Blanton 1976a; Crumley 1976; Johnson 1977). It is easily conducted with archaeological data and can be logically linked to central-place models. For these reasons, rank-size analysis has attracted considerable attention (e.g., Blanton 1976b; Crumley 1976; Hodder 1979a; Johnson 1977, 1980a,b; Kowalewski 1980).

The *rank-size relation* is the relation between the size of the settlements in the region and their rank. This relation is specified by rank ordering the settlements from largest to smallest, giving the largest place the first rank. The relationship between a settlement's rank order and its size can be graphed as a bivariate plot, commonly with the rank on the horizontal axis and the size on the vertical axis.

One expectation used to interpret these empirical plots is encompassed by the rank-size rule. The *rank-size rule* predicts a rank-size relation for all the sites in a settlement pattern, under the assumption that they are equally well integrated in a single settlement system (e.g., Beckmann 1958; Beckmann and MacPherson 1970; Richardson 1973; Vapnarsky 1969). Its formal expression is

$$S_i = S_1/R_i$$

where

$S_i$  = the size of the  $i$ th settlement in the area

$S_1$  = the size of the largest settlement in the area

$R_i$  = the rank of the  $i$ th settlement.

Although the rank-size rule was initially an empirical generalization (Zipf 1949), it has been linked to theoretical models (e.g., Richardson 1973

for a review). One particularly interesting line of argument links the continuous rank-size rule to the discrete site-size distributions expectable under central-place theory. Developed by Beckmann (1958), he notes that if the sizes of the sites within a hierarchy level in a central-place system vary around the expected size for the specific hierarchy level, the overall effect is a continuous distribution of site sizes. Furthermore, if the variation in site size results from the multiplicative effects of a number of random variables, the resultant continuous distribution of site sizes approximates the log-normal distribution. The rank-size rule is a special case of the more general log-normal distribution (Beckmann 1958; Beckmann and MacPherson 1970; Parr 1969).

Linking the rank-size rule to central-place models in this manner is particularly useful for archaeological research. For one, cultural systems are frequently modeled with random effects and, these random effects are often considered to be multiplicative (e.g., Johnson 1980a; Renfrew 1972:25–44, 476–504). Thus, Beckmann's settlement system is not inconsistent with ethnological modeling on these grounds. Furthermore, the fact that the model generates a continuous distribution out of the hierarchical central place model is methodologically useful, as the rank-size rule can be used to evaluate empirically developed, continuous rank-size relations.

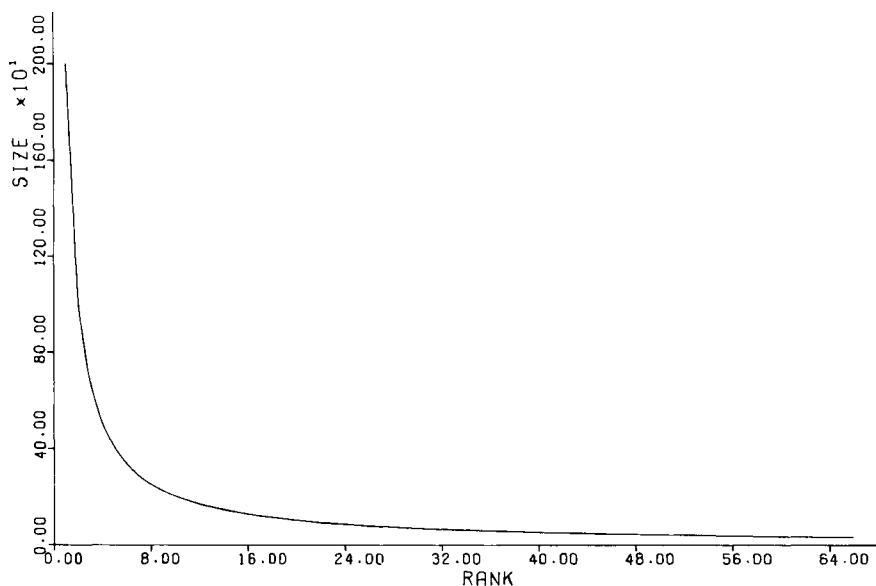


FIGURE 11.1 Rank-size rule.

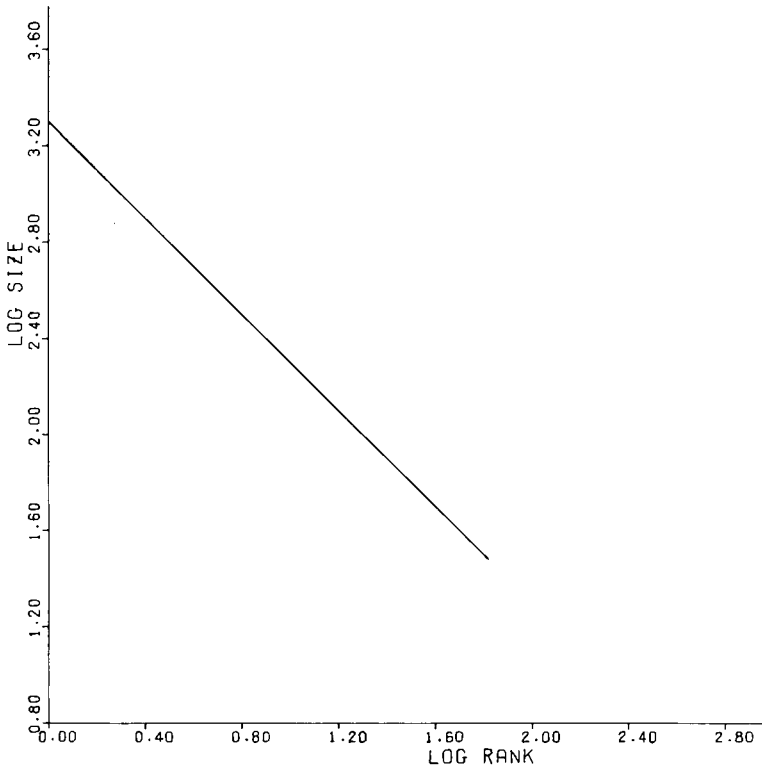


FIGURE 11.2 Rank-size rule: logarithm-rank-logarithm-size scales.

A word of caution is found in Richarson (1973). His review of rank-size relations points out that a number of different behavioral models give rise to the rank-size rule. Thus, as in any interpretive analysis, identifying the operation of central-place systems should be based on many independent lines of evidence, of which the rank-size relation is but one.

As mentioned earlier, conducting a rank-size analysis is quite straightforward. When plotted on regular graph paper, the rank-size rule—the expected distribution—appears as in Figure 11.1. Transforming the values for rank and size to their logarithms (base 10) and plotting these yields a distinctive straight line (Figure 11.2). Thus, when the empirical plot approximates the expected straight line, the interpretation that a study area contained a complete, well-integrated settlement system finds support. Various statistical procedures have been proposed to evaluate the accuracy of fit

between the expected and observed distributions (e.g., Johnson 1980b; Paynter 1982). However, as the overall shape of the deviations in the following analyses proves informative, I will not base the following discussions on these measures.

Behavioral interpretations have also been developed for some of the patterns of deviation from the rank–size rule. Three have been used in the archaeological literature. The first is a *concave* pattern of deviations (e.g., Figure 11.3), in which the observed values fall below the expected rank–size rule line. Blanton (1976b:195–201) and Johnson (1977:496–499) suggest that concave deviations arise if an archaeological study area is much smaller

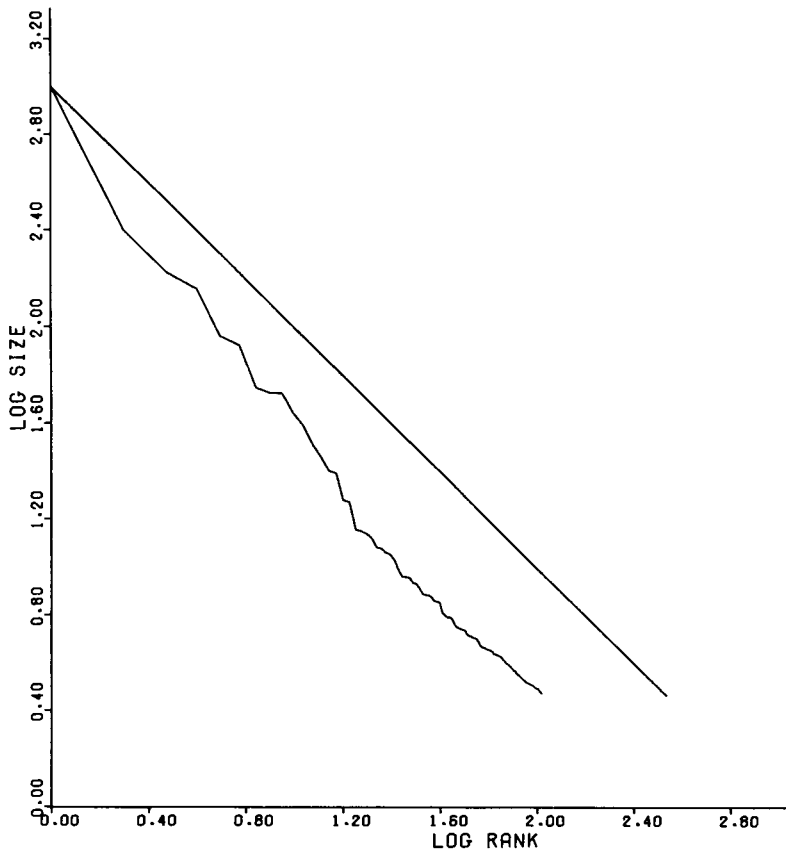


FIGURE 11.3 Concave deviations.

than the area influenced by the past settlement system. In an archaeological application, Blanton (1976b:200) uses concave plots for the Valley of Mexico to support the Valley's central position in a larger political economy during the Middle Horizon and Middle Second Intermediate periods.

The second is a *convex* pattern of deviations (e.g., Figure 11.4), in which the observed plot falls above the expected plot. Two behavioral processes have been suggested to account for this pattern. One suggestion is that convex deviations arise when two or more complete, well-integrated systems are pooled in the analysis (e.g., Johnson 1977:499). The other suggests that areas located in the peripheries of larger political economies may also display convex deviations (Paynter 1982). Johnson (1980a) identifies an essential similarity in these situations, arguing that both peripheries and pooled study areas represent situations of poor system integration. More attention is paid to convex patterns of deviation in the following, particularly regarding discrimination between pooling and peripherality.

The third pattern of deviations is the *primo-convex* pattern (e.g., Figure 11.5). Johnson (1980b) identifies this pattern, interpreting it as resulting from a "settlement system composed of sub-systems (enclaves) which are articulated with a regionally dominant center but which are relatively independent of one another." The Warka area during the Early Uruk period exemplifies these processes (Johnson 1980b).

In sum, rank-size analysis is an attractive interpretive tool. It enables one to analyze the rather poor data from archaeological surveys with rich central-place models. Furthermore, it is easy and inexpensive to conduct.

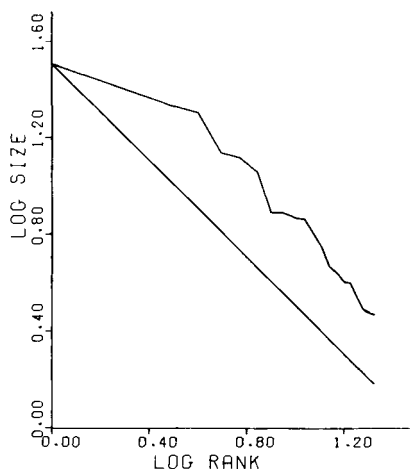
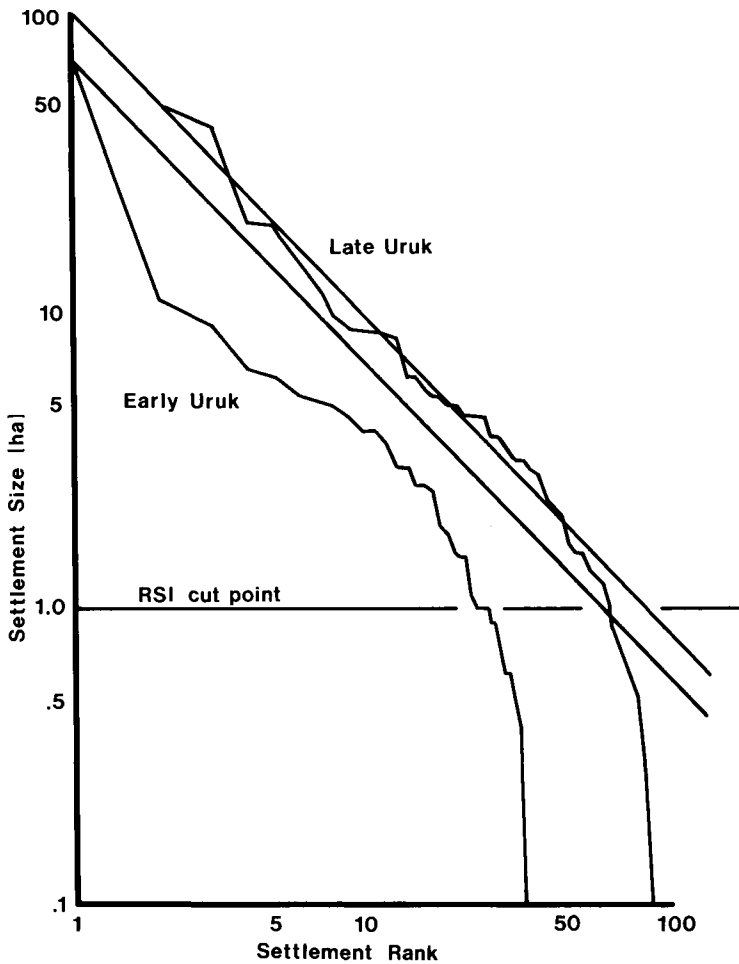


FIGURE 11.4 Convex deviations.



**FIGURE 11.5** Empirical primo-convex deviation pattern from the Warka study area, Iraq (from Johnson 1980b). The curve for the Early Uruk period exemplifies a primo-convex deviation. The RSI cut point is the probable minimum settlement size for economic viability.

These are precisely the characteristics that also make it a tool that may be misapplied if attention is not paid to the limits of rank–size analysis, and the underlying central-place models. The next section considers some of these limits.

## BIASING PROBLEMS, BOUNDARY PROBLEMS, AND STRATIFICATION

Although central-place models are useful tools for interpreting archaeologically recovered site hierarchies, there are limits to their applicability. The theory of conventional central-place models is too narrow to be facilely applied to the archaeological record. Furthermore, methodological problems arise because these models are designed to analyze the relatively rich, well-controlled data of the contemporary world. A number of recovery problems complicate their application to prehistoric settings.

In the following, I investigate three problems. The first results from biasing processes. The problem occurs because archaeologists do not control preservation processes; thus, it is unlikely that the full settlement pattern from a previous system will ever be recovered. This problem and means of compensation are illustrated with rank-size analysis.

A second problem also arises from the data. Archaeologists use rather arbitrary analytic units in collecting survey data. There is no assurance that the analytic system bounds an area containing a past cultural system. In fact, as I argue later, it is quite unlikely that survey units contain a settlement system. Again, rank-size analysis is used to illustrate this problem and suggest some procedural remedies.

A final problem in using central-place models for interpretation of past cultures concerns the relevancy of the behavioral assumptions found in these models. This is not a new criticism of central-place models. However, in the following I do not argue for the irrelevancy of these models; but, rather I suggest expansion of the relevancy of this approach by considering the allocation of the costs associated with overcoming distance in different political economic settings. I close with some suggestions about the role of spatial relations in maintaining the relations of inequality found in stratified political economies.

### Biasing Problems

A basic problem in any archaeological research, and particularly in studying past settlement patterns, is the issue of bias. Even if we could decide what a site is (e.g., Chang 1972; Dincauze 1978; Hodder and Orton 1976:18–20), it seems highly unlikely that 100% would survive the variety of geomorphological processes and land uses impacting any area. Thus, it is unlikely that an archaeologist ever analyzes a complete settlement pattern.

However, most surveys fairly routinely reconstruct past patterns and evaluate the goodness of fit of various formal settlement models without

giving much systematic thought to the effects of these biasing processes. Note that I am not considering issues in sampling strategy or design. These are certainly important issues and have received considerable attention in a number of reviews (e.g., Hole 1980; Mueller 1974, 1975; Plog 1976a; Plog *et al.* 1978). In the following, I presume sagacity in allocation of labor and money and I consider samples arising from incomplete preservation. I am concerned with the situation in which, after having conducted a 100% survey of an area, with appropriate intensity, an archaeologist suspects that some components of the past settlement pattern have been eliminated from the record. Obviously, this has to be taken into account, especially when using formal analyses, such as rank-size interpretations, which assume a total system. Since these preservation problems are different in each study area, the best strategy is unique to each setting.

Surveys often report on biasing processes (e.g., Johnson 1973:24–27; Parsons 1971:19–20; Price 1978:213; Sabloff and Rathje 1975:62–63). For example, Sanders *et al.* (1979:20–30, 60–65) present the range of activities that biased their sample of sites in the Basin of Mexico. These include geomorphological processes, such as erosion and alluviation, and contemporary land-use patterns, such as urbanization and heavy crop cover, hostile humans and hostile canines—problems and experiences quite familiar in any survey setting. So are the following observations:

We have never dealt systematically and rigorously with these potentially serious problems. However, we have always been aware of these difficulties and have tempered our observations in view of the more obvious limitations of our method. . . . While we are prepared to admit that problems of differential modern land use and seasonal variation have almost certainly produced some inconsistency and error in making inference about occupational density and site character, we believe that such errors and inconsistencies have not significantly affected our general conclusions [Sanders *et al.* 1979:64–65].

I leave the evaluation of the last conclusion to those more familiar with the area, and address the issue of introducing some rigor into considering the effects of biasing processes.

One method of coping with bias is to formally model the *aggregate* effect of all biasing processes. This involves using limit theorems, and thus assumes that past biasing processes were random and either numerous or frequent. For instance, Dacey describes one such theorem:

If artifacts deteriorate, are destroyed or are removed with constant probability, this theorem suggests that after a sufficiently long time the spatial distribution of the remaining artifacts will have a Poisson pattern [1974:3].

The strategy, in this case, is to compare the observed pattern against the Poisson under the suspicion of past biasing processes. A lack of fit to the Poisson would be encouraging, suggesting that some behavioral effects still remain. A similar strategy has been used by Hodder (1979a) in studying rank-size relations. He uses the normal distribution as the limit for a sampled hierarchy, acknowledging that this is not necessarily a well-founded assumption.

However, there are problems with studying aggregate effects that limit its practical utility, despite its mathematical elegance. Specifically, one seldom suspects that the biasing processes in the past were either many or random. In fact, one usually suspects that there was a distinct bias toward sites of certain characters or sites located in specific areas. Thus, the assumptions of limit theorems do not accurately model the past processes.

A more informative strategy is to test the strength of the relationship between archaeological data and the suspected biasing process(es). For instance, Hodder and Orton (1976:226) present Kolmogorov-Smirnov tests for evaluating the strength of the relationship between artifacts (Late Iron Age coins) and other landscape features, in this case Roman roads in central and southern England. Of course, Hodder and Orton's analysis proposes that old roads conditioned the deposit of old coins. However, this can be turned around to evaluate the proposition that contemporary roads positively bias the identification of artifacts or sites. This basic approach could easily be extended by identifying other landscape features likely to affect site recovery, such as erosion and alluviation surfaces, or zones of varying land-use intensity.

### *Simulating the Biasing Effects*

A third approach to studying the effects of bias is simulation of the combined effects of the past behavioral and biasing processes. To do this, one needs to posit the past system, as well as the effects of hypothetical biasing processes. This strategy can be illustrated with rank-size analysis. The rank-size rule fulfills the first condition by providing an expectation for an unbiased settlement pattern. Let us consider what deviation patterns from the rank-size rule are likely under various biasing processes.

A few simple thought experiments using the rank-size rule suggest some of the effects. For instance, arbitrarily divide the sites in the rank-size bivariate plot into three groups: the larger places, the middle-sized places, and the smallest places. Eliminating the larger places leads to markedly convex deviations as this results in too few large places. If middle-sized places are lost, the pattern of deviations conforms to the rank-size rule for large places and becomes concave as there are too few smaller places (John-

son 1977:496). Missing small places leads to overall conformity to the rank–size rule with a concave “tailing off” in the lower reaches. Thus, some very simple rules of thumb emerge. Convexity may be due to biases affecting large places, such as the development of contemporary urban places. Concavity may be due to effects in middle-sized places; and, concave “tailing off” may be due to elimination of small sites, such as through erosion and/or alluviation.

However, empirical situations are likely to be much more complex. First, some combination of large, middle-sized, and small places are likely to have been affected. Second, not all places at a given level are likely to have been affected. These more complex situations, especially when there is uncertainty about numbers of affected sites, can be studied with computer simulations.

The following is an example of one simulation. The simulation starts with the assumption of a complete, well-integrated settlement system in which the largest place was arbitrarily set at the size of 1000. The system is based on the transport principle, thus following a  $k = 4$  lattice (Christaller 1966:74). The system has four tiers, thus exceeding one of Wright and Johnson’s (1975:267, 270) characteristics of state settlement patterns. Each of these tiers was sampled randomly without replacement.

Different biasing situations were simulated. Table 11.1 specifies the number of places eliminated from each tier. The situations range from such qualitative judgements as, the survey recovered all but the small places (strategies a and b), to urban sprawl, erosion, vulcanism, agricultural land-use, pot hunters, and previous archaeologists got some of the larger sites as well as some of the smaller sites (strategies h and i).

The results are plotted in Figure 11.6. Three lines appear on each plot (most clearly seen in Figure 11.6g–i); a solid straight line for the underlying total system; a curved line with + for the observed sites after past processes sampled the total system; and, a straight line with + indicating the rank–

**TABLE 11.1**  
Biasing Simulations: Number of Places Sampled from Each Tier of a 4 Tier, K4 Central Place System

Tier	Total Number	Number sampled								
		a	b	c	d	e	f	g	h	i
1	1	0	0	0	0	0	0	1	1	1
2	3	0	0	0	0	1	1	0	1	1
3	12	0	0	0	1	3	2	1	3	2
4	48	5	10	24	5	14	24	5	14	24

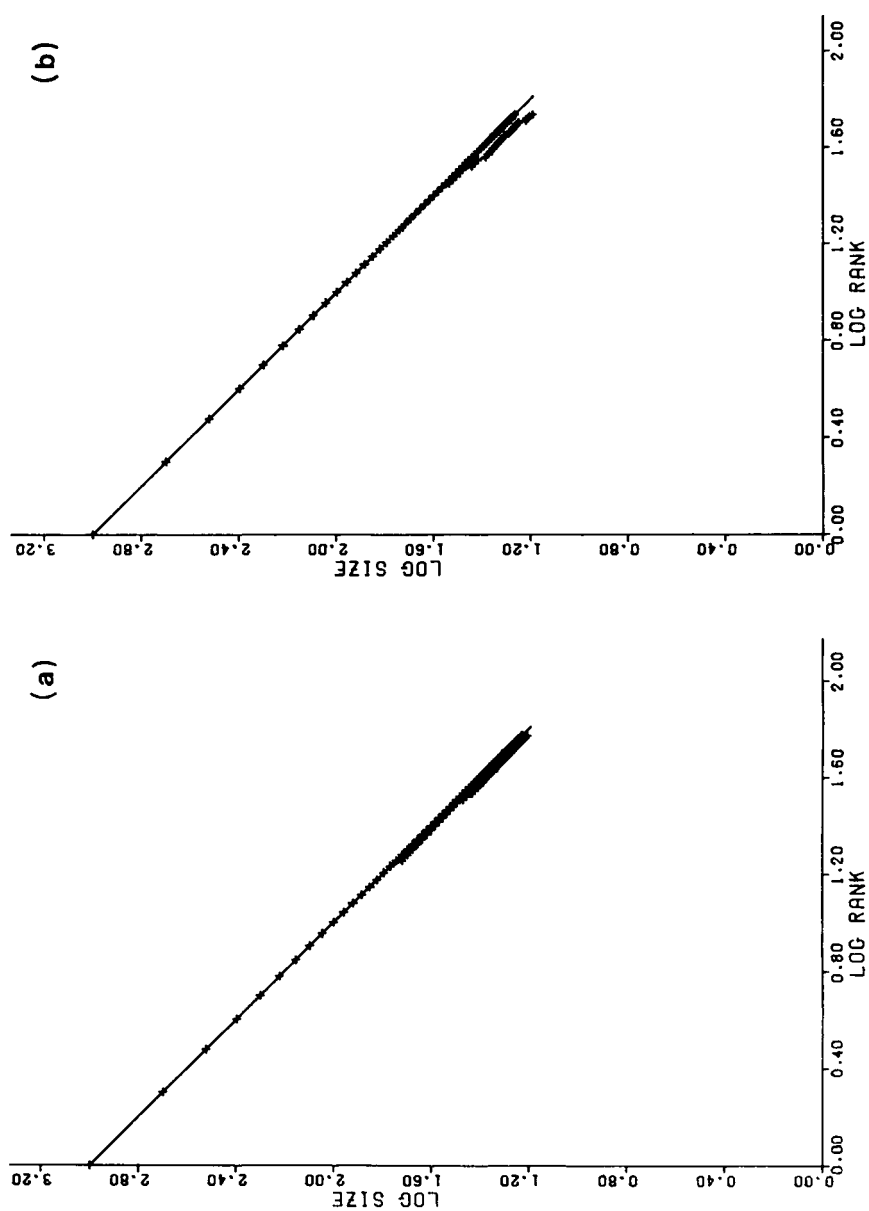


FIGURE 11.6 Biasing simulations (see Table 11.1 for descriptions of the samples).

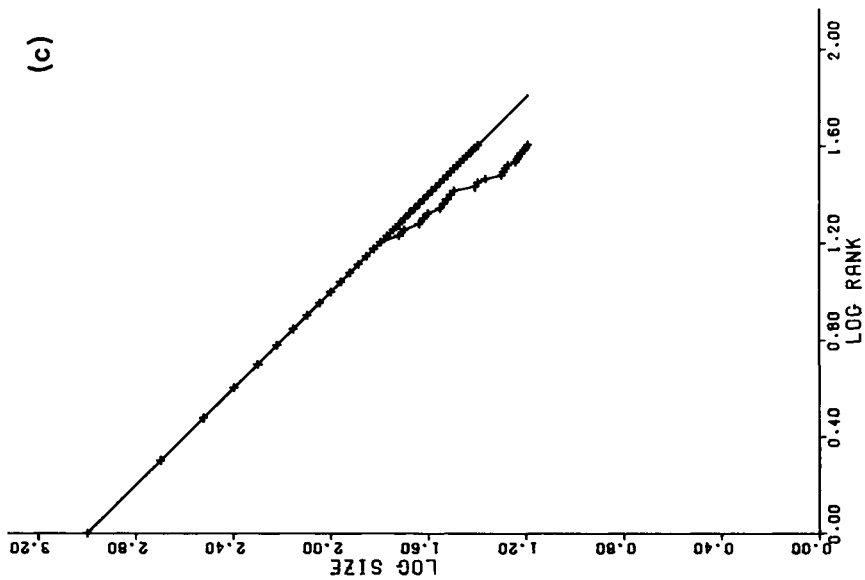
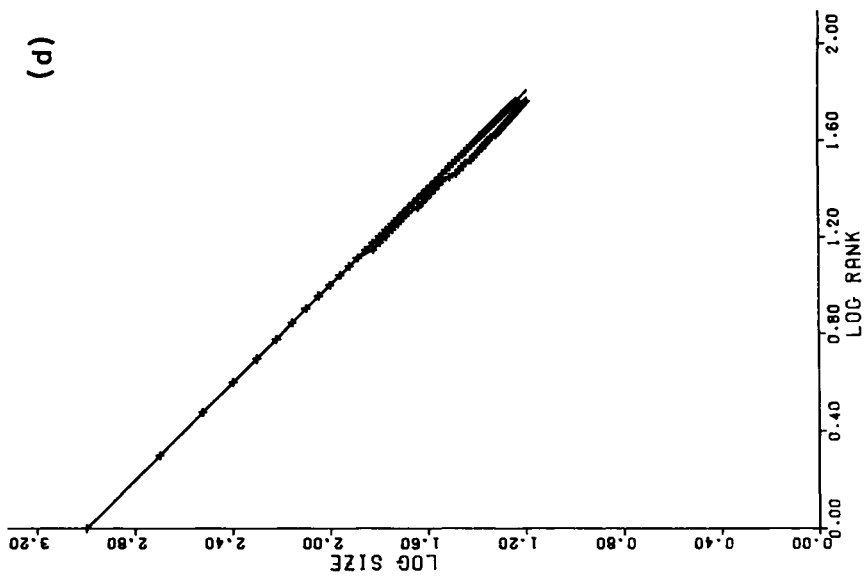


FIGURE 11.6 (cont.)

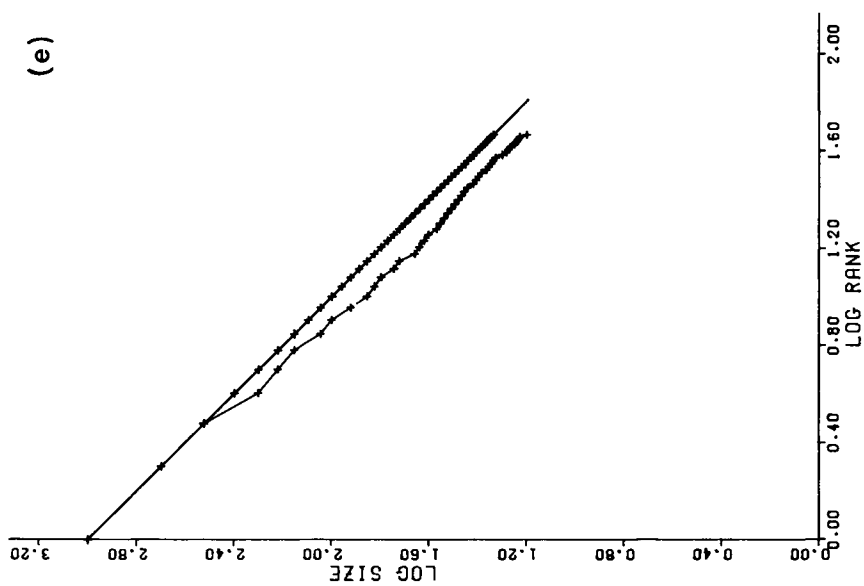
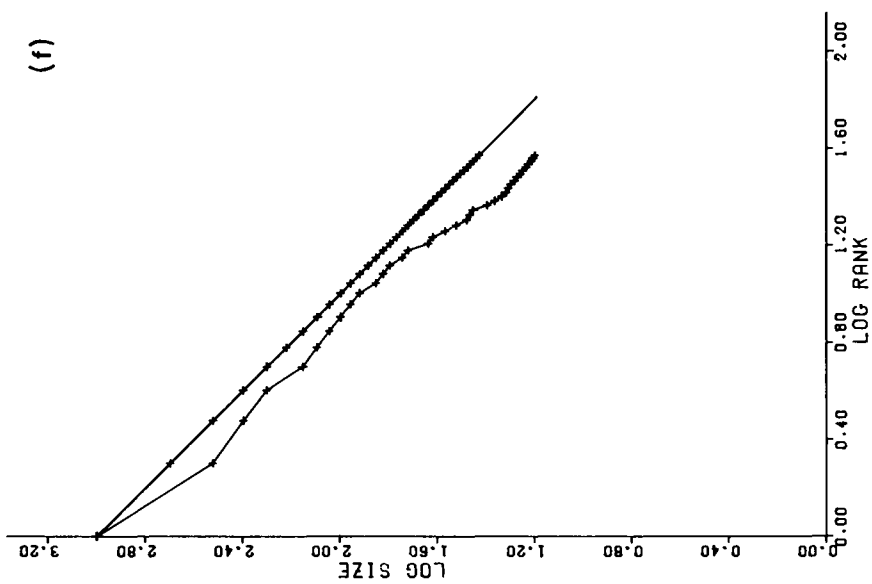


FIGURE 11.6 (cont.)

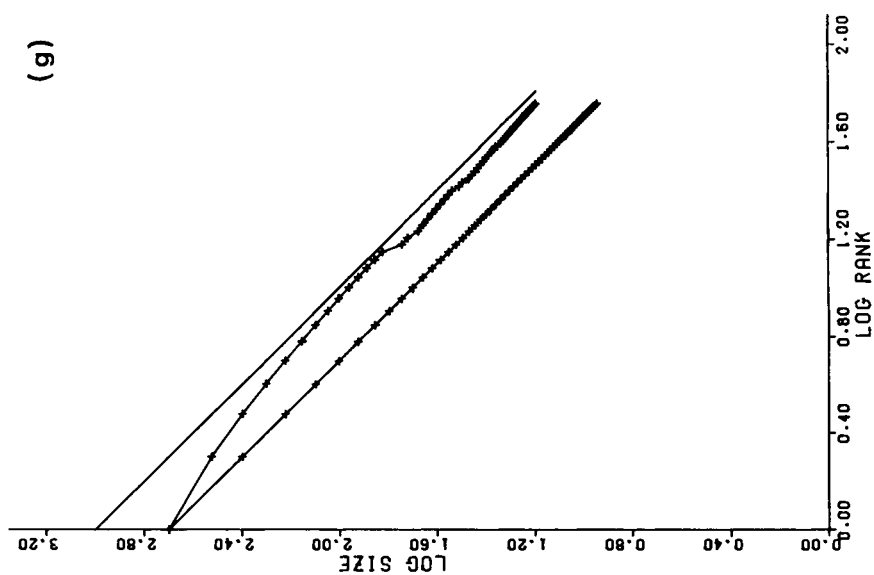
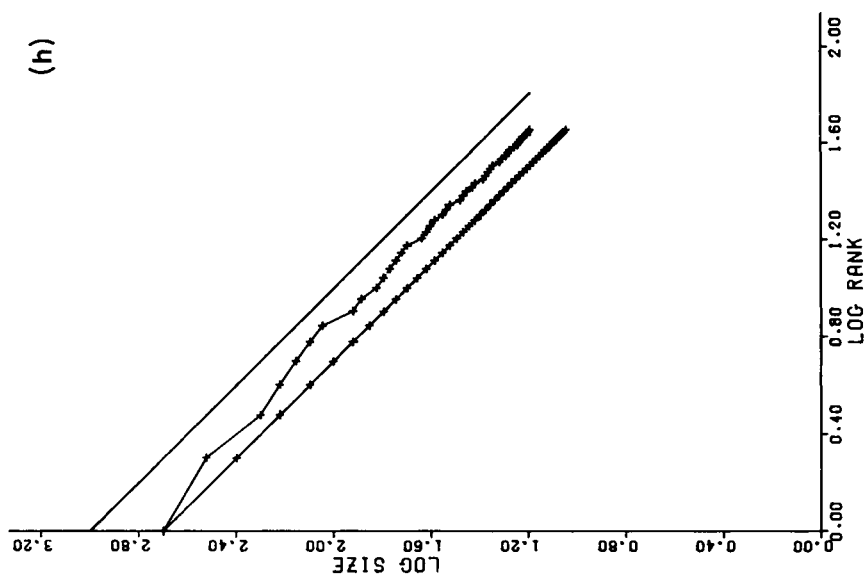


FIGURE 11.6 (cont.)

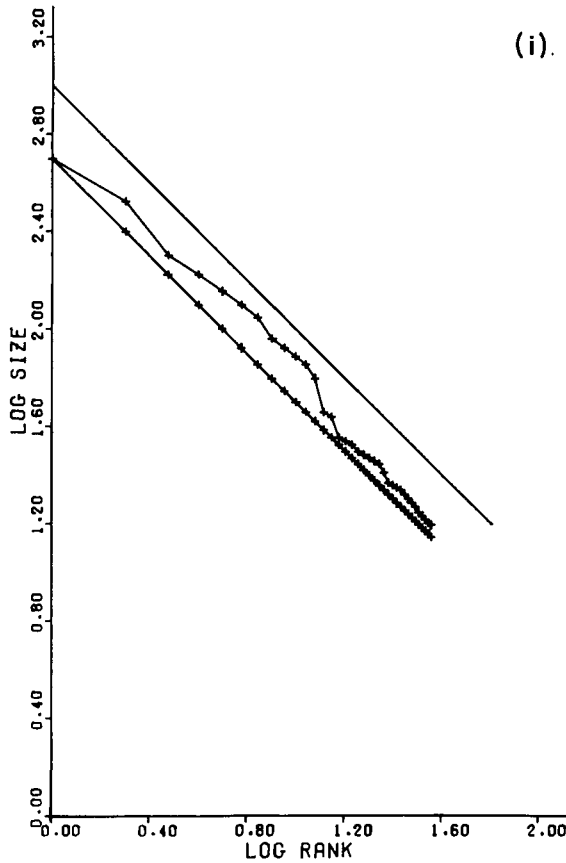


FIGURE 11.6 (cont.)

size rule an archaeologist would construct with this sample. In situations of minor biasing, all three lines are nearly superimposed (e.g., Figure 11.6a).

Quite expectably all the biasing experiments deviate from the rank-size rule. More interesting, if these processes affect only small places, the pattern of deviations is not likely to complicate interpretations (e.g., Figure 11.6a–d). This is partially because the logarithm–logarithm transformations lead to small graphic changes when highranked places are removed. However, the loss of larger sites strongly affects the pattern of deviations. As more large sites are eliminated (Figure 11.6e,f) the deviations become concave. If the largest settlement is affected (Figure 11.6g–i), convex deviations are produced.

The convex and concave patterns of deviations create obvious interpre-

tive problems. For instance, an empirical concave plot (e.g., Figure 11.6f) is generated by the biasing effects modeled as in Table 11.1, and, as presented previously, by boundary effects. An empirical convex plot (e.g., Figure 11.6g) is generated by severe biasing problems, by boundary effects and by low systemic integration. Given that different processes can produce similar deviation patterns, care should be taken to rule out alternative explanations when interpreting rank–size plots.

A further implication of these simulations is that other familiar analytic procedures may also be affected by biasing processes. For instance, nearest neighbor assessments (e.g., Earle 1976; Hodder and Orton 1976:38–51), analyses of intersite stylistic patterning (e.g., Plog 1976b, 1980; Voss 1980), or determinations of settlement hierarchy are also likely to be affected. Further research should be conducted to determine the kinds of biasing procedures most likely to create problems.

In sum, it is safe to assume that a total settlement pattern will never be recovered. The landscape of a study area is best considered to be a continually evolving surface transformed by past cultural systems of interest, by intervening cultural systems, and by noncultural factors. Thus, models of patterns derived from central-place theory should differ from empirical patterns. Biasing, in particular, might be an important factor, if land-use practices have led to incomplete data on large settlements. However, rank–size analyses can be conducted by using hunches about missing data to construct sampling simulations as presented here. The resultant rank–size relations, rather than the rank–size rule, are the expectation for the incomplete, well-integrated central-place pattern.

## Boundary Problems

A second problem in applying central-place models concerns bounding theoretical and analytic units. Theoretical central place models predict the landscapes of one spatially open system. The empirical situation departs from the model since a study area, for a specific period, may contain many small-scale systems, part of one larger-scale system, or part of two (or more) larger-scale systems. Rarely, however, will an empirical study area contain the pattern provided by just one system. This discrepancy between analytic and systemic boundaries has been referred to by Johnson (1977:489) as the boundary problem.

There are theoretical and methodological reasons why the boundaries of the analytic unit and the past system are not likely to coincide. Johnson (1977:498–499) identifies the arbitrary nature associated with archaeological boundary decisions as a methodological source of this problem. For

instance, Adams and Nissen (1972:5) point out the controlling effects of contemporary land-use and political subdivisions on their study area. Furthermore, recent research on the spatial scale of previous sociocultural systems suggests that archaeological study areas are not likely to coincide with past systems. Johnson (1975:294) notes that study areas of stratified societies tend to range between 2,000 and 10,000 km<sup>2</sup>. Such areas would seem large enough to encompass earlier sociocultural systems under the received wisdom of cultural evolution. One of the major evolutionary trends is believed to be an increase in spatial scale with increasing sociocultural complexity (e.g., Beardsley *et al.* 1956; Mosely and Wallerstein 1978:261; Steward 1955:170–209). Study areas used in archaeological survey would certainly contain the interactions of households, as suggested for prestratified societies (e.g., Coe and Flannery 1964; Steward 1955:201–202), and even of stratified societies organized as Early State Modules of about 1,500 km<sup>2</sup> (Renfrew 1975:14).

However, there is good reason to suspect that the evolution from relatively egalitarian systems to stratified systems is *not* paralleled by a change from small arenas to large arenas in any simple fashion. It is abundantly clear that some band societies, either in the ethnographic present or in prehistory (Lee 1979; Wilmsen 1980; Wobst 1976) operate in quite large arenas. The isolated tribe model no longer seems viable in light of Fried's (1975) critique and reports analyzing big-man exchange systems in New Guinea (e.g., Harding 1967; Malinowski 1961; Meggitt 1974; Paynter and Cole 1980). Furthermore, students of the early modern and contemporary stratified societies that emerged in northwestern Europe and North America find that it is not very useful to view nation-states as closed systems (e.g., Peet 1969, 1972; Schneider *et al.* 1972; Wallerstein 1974, 1980). The only remaining closed evolutionary type is the archaic civilization (e.g., Renfrew 1975). I would expect its closedness to fast go the way of the closed culture concept in these other areas, once attention is devoted to identifying its scale rather than assuming small spatial arenas (e.g., Johnson 1980b; Kohl 1978; Price 1977).

Obviously, if past social systems operated in arenas exceeding the size of survey units, it would be impossible to have analytic and theoretical boundaries coincide. The settlement patterns under analysis would be the product of part of larger settlement systems, or if situated at the border of a number of these past systems, might be parts drawn from more than one. Thus, it seems theoretically as well as methodologically unlikely that patterns under investigation represent one system. Using models of settlement systems that presuppose the operation of one system might be quite misleading.

In the same article in which he identifies the boundary problem, John-

son (1977:498) also offers spatial analysis based on the rank–size rule as a procedure for sorting out the fit between empirical study areas and theoretical units. Specifically, Johnson (1977:498) suggests that if two past systems are pooled within one study area, convex deviations from the rank–size rule should result. Alternatively, concave deviations are suggestive of the study area capturing only part of the relevant settlement pattern (see also Blanton 1976b:199–200; Vapnarsky 1969).

A problem for using deviations from the rank–size rule in identifying the boundary problem is that there are other interpretations for these patterns. For instance, as noted previously, deviations may also arise when biasing processes have eliminated some of the larger sites. And in yet another case, convexity is associated with drawing samples from the periphery of larger systems.

I became particularly interested in sorting out these different interpretations during my study of the settlements in the nineteenth-century Connecticut River valley (Paynter 1980). Documentary sources indicate that the Connecticut River valley was an agricultural semiperiphery in the late eighteenth century and became more of a core-like industrial area by 1850 (e.g., Henretta 1973; Lemon 1980; Wallerstein 1974, 1980). Rank–size analyses were conducted using documentary data on three indices of town size. Being a sample of a larger system, the patterns should have been concave. This was not the case.

Figure 11.7 displays one of the convex rank–size relations for population density. For reasons of space, I do not include results of other analyses. They, as with Figure 11.7, are markedly convex.

Aside from the independent historical information indicating that this pattern of deviation is associated with peripherality, it was possible to rule out some of the competing hypotheses. First, biasing was not responsible since the documentary data covered the entire study area. Second, subdividing the area into the most likely smaller independent systems did not generally produce good fits to the rank–size rule—a predictable pattern if in fact the study area pools smaller independent systems. Both of these observations could be made with data from a prehistoric survey; thus, it is possible to distinguish convexity associated with pooling and convexity associated with peripherality.

In search of yet another diagnostic to distinguish peripheral and pooling convexity, simulations of pooling were conducted and the pattern of deviations studied and compared to the patterns from the Connecticut River valley. Four instances of pooling were considered. In the first (Figure 11.8a) two identical patterns were added in the same rank–size analysis. The patterns were derived from complete, well-integrated systems and therefore, followed the rank–size rule. The largest place in each was arbitrarily set at

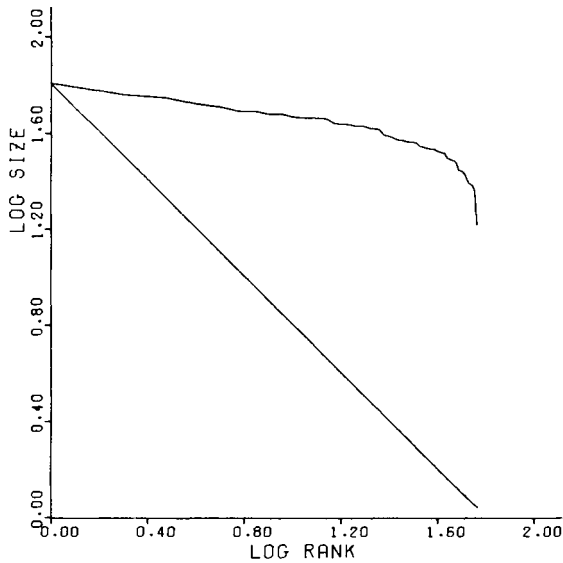


FIGURE 11.7 Peripheral rank-size relations from the Connecticut River Valley.

1000; each followed a  $k = 4$  lattice, and each had four tiers. The other analyses pooled patterns of different systems with this 1000,  $k = 4$ , four-tiered system. The second analysis (Figure 11.8b) analyzes two systems with the same largest place but different lattices and number of tiers (i.e., a different structure). The third analysis (Figure 11.8c) pools two systems with different-sized largest places, but identical structures. The fourth (Figure 11.8d) pools systems with different largest places and different structures. The system characteristics are found in Table 11.2.

A brief aside helps interpret the graphs. Four lines occur on each plot, most clearly seen on Figure 11.8c and d. Two straight lines with + are the plots of the independent patterns (in Figure 11.8a and b, these are superimposed). Two solid lines are the expected rank-size rule and the observed rank-size relation resulting from pooling the two independent systems. In these experiments, the pooled expected plot (the straight solid line) will always be superimposed on the plot of the independent system with the largest first rank place.

There are a number of interesting characteristics of these deviation patterns. First, the expected convexity appears in all four types of pooling. Second, compounding systems with different-sized largest places leads to

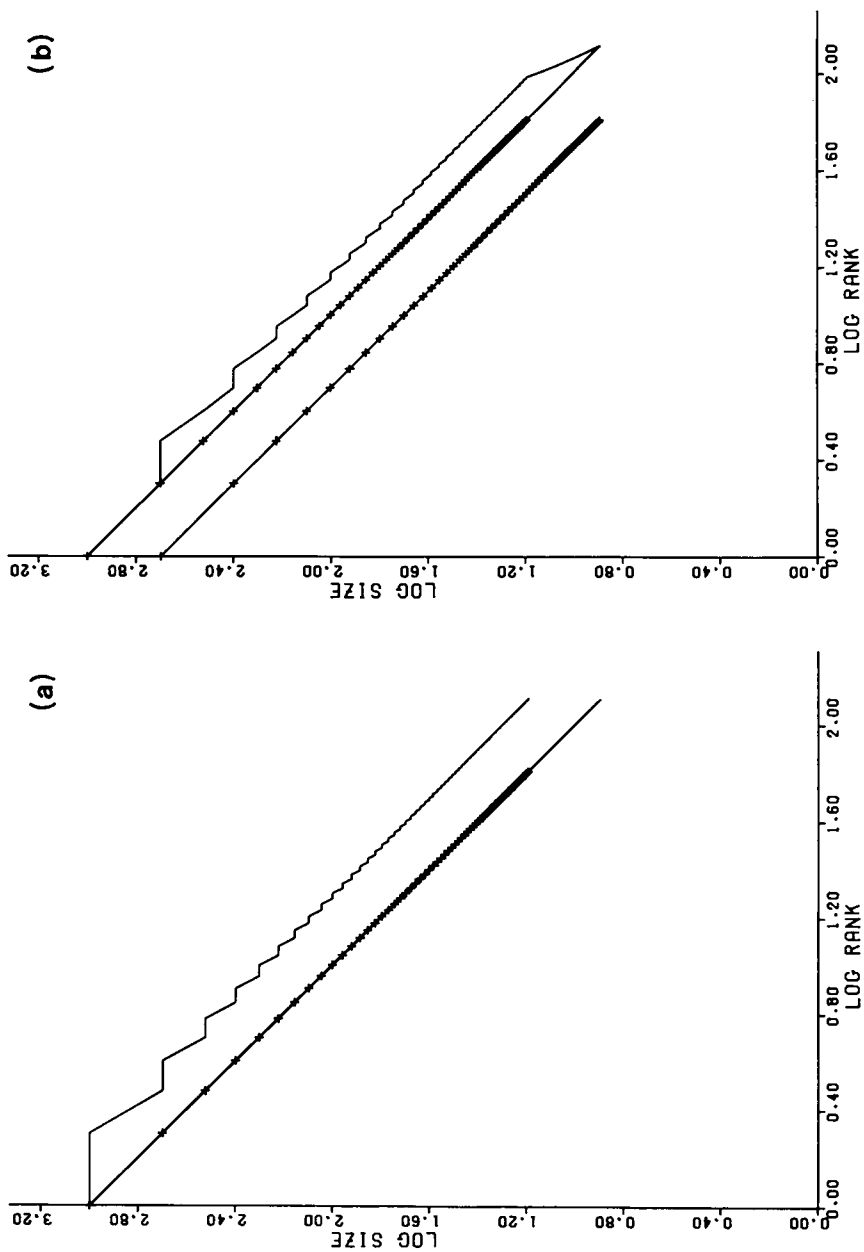


FIGURE 11.8 Compounding simulations: (a) two identical systems; (b) different largest places, same structures; (c) same largest places, different structures; (d) different largest places, different structures. (See Table 11.2 and text for descriptions of the systems.)

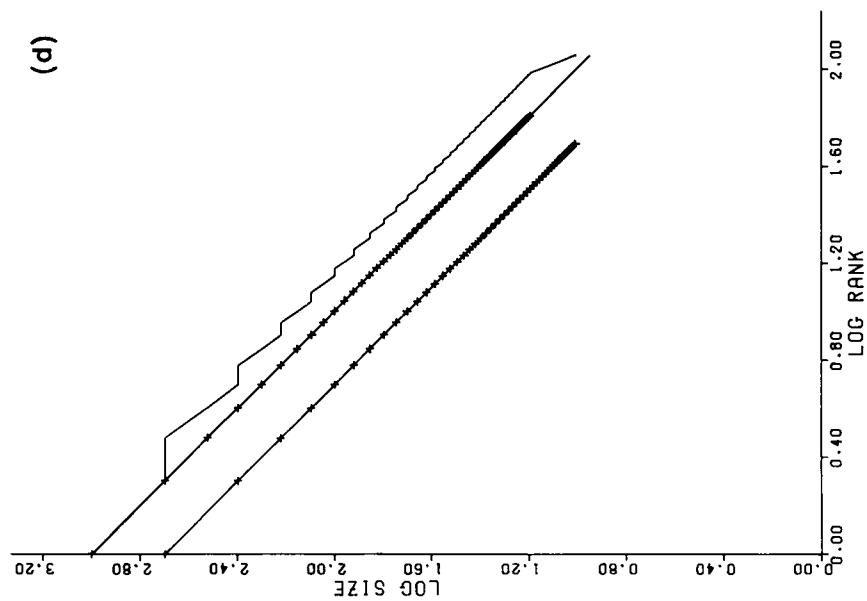
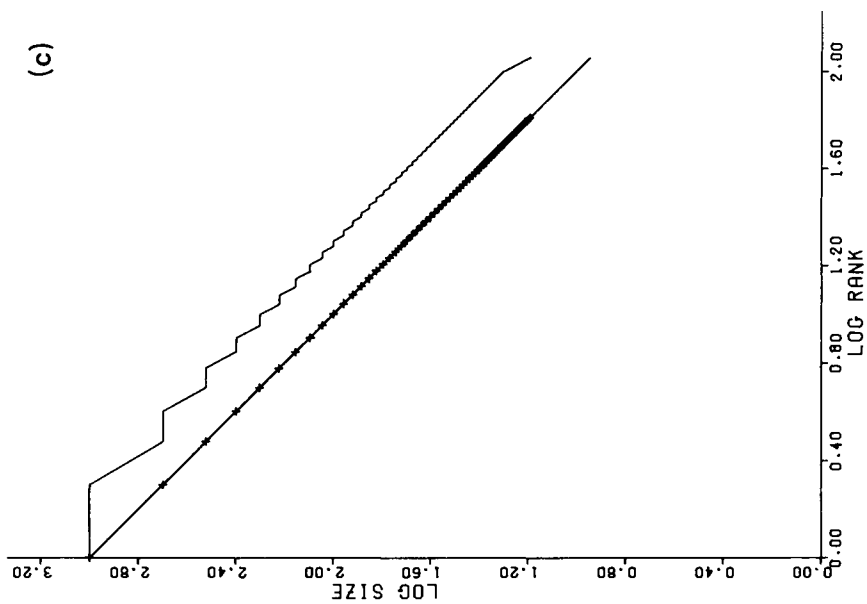


FIGURE 11.8 (cont.)

**TABLE 11.2**  
System Characteristics in Compounding Experiments

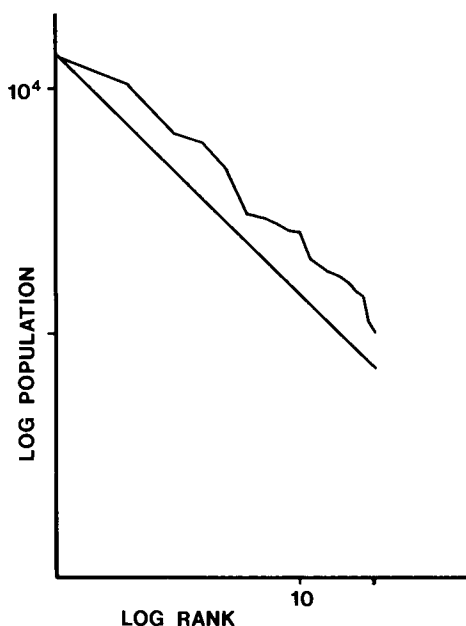
	(a) Identical patterns		(b) Same size, different structure	
	System 1	System 2	System 1	System 2
Largest place	1000	1000	1000	1000
<i>k</i>	4	4	4	7
Number of tiers	4	4	4	3
	(c) Different size, same structure		(d) Different size, different structure	
	System 1	System 2	System 1	System 2
Largest place	1000	500	1000	500
<i>k</i>	4	4	4	7
Number of tiers	4	4	4	3

conformity between observed and expected in the lower ranks (larger places). Third, when the systems differ in either structure or size of the largest place, the observed line has a slight “tailing-off” in the higher ranks (smaller places). Finally, the convex deviations are roughly parallel to the rank–size expected line.

The characteristic most useful in distinguishing pooling from peripheral convexity is the roughly parallel character of the observed and expected lines. This contrasts with the patterns from the peripheral Connecticut River valley, which though convex, are strikingly nonparallel (Figure 11.7).

Obviously, these cases do not cover all possible instances of pooling. Thus, as suggested previously in the biasing process discussion, an investigator suspecting pooling might produce expected rank–size deviations based on hypothetical systems and use this to evaluate the observed rank–size relation. An example from Oaxaca illustrates this. Kowalewski (1980) argues that during Monte Albán V, several independent polities occupied the valley. His rank–size analysis discloses the convex pattern. Consistent with the simulation of two, roughly equivalent systems (Figure 11.8a), the empirical pattern discloses a roughly parallel pattern (Figure 11.9).

Interestingly, Kowalewski’s case of pooling in the Valley of Oaxaca might be consistent with the revisions in evolutionary scale that began this discussion. It may be that pooling of independent systems within an archaeological study area is most likely in periods of state system collapse. Prior to the emergence of the state, political economic institutions lacked the ability to establish impermeable boundaries (e.g., Hodder 1979b). With the



**FIGURE 11.9** Compound rank size relation from the Valley of Oaxaca, Monte Albán V (from Kowalewski 1980).

emergence of the monopolization of force, the political apparatus of the state is available to regulate and enforce exchanges to the (heretofore unknown) extent of closing down exchanges.

Regardless of the eventual outcome of models for state cyclicity, the issues of scale and boundaries are clearly problematic. Rank-size analysis should prove useful in identifying boundary mismatches, if other interpretations can be excluded. A key may be parallel versus nonparallel convex deviation patterns. However, truly coming to grips with the boundary problem means carefully investigating the scale of precapitalist systems. Present evidence suggests that many systems operate at surprisingly large scales—exceeding the traditional scale of archaeological survey units. Since analytic and systemic boundaries are not likely to coincide, studying these units with inappropriately large central-place models distorts the understanding of these past systems. A strategy for coping with the boundary problem is to sample and pool central-place systems, using the resultant patterns as guides for interpreting empirical patterns.

## Behavioral Models and Patterns

A third limit in using central-place models in archaeology concerns issues of theory. How useful are central-place models for interpreting past cultural systems and for developing interesting ethnological theory? Even if the observational problems and the methodological problems discussed previously were overcome, would archaeologists, as social scientists, find settlement models based on central-place theory useful?

Flannery (1976:169–170) points out that criticisms of central-place models are often misdirected toward the assumption of uniform plains. The more important issue for the development of social theory concerns the societies and agents underlying these models. Two of the more important deficiencies of conventional central-place models involve the assumptions about social systems, and the effects of these social systems on the material world. Specifically, most models assume the action of rational men, an assumption that limits the ethnological interest of these models. Second, the models predict economic exchange activities rather than physical settlements. From the archaeologist's perspective, the models dangle above the ground. Keeping these aspects of conventional central-place models obscures the past, leads to irrelevant debates, and contributes to the misuse of settlement models. Considering these issues, however, points to ways to expand the scope of settlement models.

First, consider how central-place models are usually grounded. As noted previously, central-place models predict the location of economic activities—usually exchange. The link between exchange and the archaeological data is primarily based on correlations between the number of economic functions and population size, on one hand, and population size and the site area on the other (Johnson 1977:495). Obviously, correlations are not behavioral theory. People do not precipitate sites; rather, sites are built, abandoned, and destroyed within specifiable social relations.

Recent developments suggest that settlement size is strongly related to settlement age and to social factors, such as wealth distribution (Kramer 1979, 1980). What is called for are models following up on these relations of class, demography, and longevity, specifying what conditions surround site construction, abandonment, and destruction. Most generally, this involves a different approach to economic functions. Rather than being concerned with exchange, consider economic functions of production. From the point of view of production, sites are made up of factors of production (e.g., buildings, warehouses, factories, craftshops, domiciles). Site size and location represents how the past system built, destroyed, and abandoned large

fixed means of production that contributed to the reproduction of stratified social relations. Developing a production approach rather than an exchange approach to economic functions would minimally involve linking site construction with such social conditions as the mode of surplus extraction, processes of administration, and ideologies of legitimacy.

What might these systems look like? One approach is to posit universally applicable assumptions that abstract human behavior to the point where formal mathematical systems can be used to model settlement patterns. The rank–size rule represents one such formalization; others include many probabilistic models, such as broken-stick models (Hodder 1979a), social physics models (Renfrew 1979:31–32), distance-decay and gravity models (Renfrew 1975), and varieties of settlement growth models based on stochastic growth processes (e.g., Hodder 1979a). This strategy, however, is a circuitous route to interesting ethnology. Johnson (1977:500–501) points out the problems with these rather bloodless models:

Yet enthusiasm for stochastic processes may go too far. . . . If the most probable state of a variable or system is equated with maximum entropy, the behavioral patterns reflected in spatial regularities are viewed as most-probable steady states resulting from purely random processes. . . . These approaches encourage taking regularity as given and shifting the emphasis of explanation to deviations. At least, in archaeology we may profitably continue to try to understand the factors producing the probability distributions that make most-probable states most probable.

Thus, when all the math is done, we are still left with the question of what ethnologically interesting behavior is modeled by these stochastic distributions.

A more common approach to central-place modeling populates a model landscape with rational men, independent, omniscient, and equally interested in maximizing profits or minimizing losses (e.g., Isard 1956: 24–54; Lloyd and Dicken 1972:9–29; Lösch 1967:92–100). These rational actors compete within the familiar institutional matrix of free markets and modern nation-states. There are a number of reasons why direct behavioral analogies are not likely to be useful.

For one, it is important to keep in mind the data central-place models were initially directed toward, namely core areas of capitalist political economies (e.g., Christaller 1966). The behavioral institutions used to generate various models (such as the marketing, transporting, and administrative principles) are those of core capitalist political economies. Critiques of the universal applicability of core capitalist institutions caution that the separation of economy and politics paralleled in the central place literature by the differing interpretations given to  $k = 3$ ,  $k = 4$ , and  $k = 7$  lattices, is unlikely in preindustrial and peripheral social formations (e.g., Amin 1980; Godelier

1977:15–62; Hindess and Hirst 1975; Polanyi *et al.* 1957; Sahlins 1968, 1972; Service 1975). This ethnographic narrowness should make us wary of arguing from these patterns to these institutions when outside of capitalist cores. Thus, it seems beside the point to ask if politics or commerce was responsible for a given pattern (e.g., Evans 1980; Smith 1979). The more ethnologically interesting problem is to consider the range of combinations of politics and economics appropriate for noncapitalist settings, also leading to  $k = 3$ ,  $k = 4$ , and  $k = 7$  lattices. Helpful empirical data and models may be found in the realms of the geography of peripheral areas and contemporary socialist landscapes (e.g., French and Hamilton 1979).

There is yet another problem with formalist assumptions. These formulations fit Renfrew's (1979:31–32) notion of social physics models, in that all actors have similar characteristics. The crucial characteristic driving the settlement system is that all actors attempt to minimize the cost of overcoming the friction of distance (e.g., Lloyd and Dicken 1972:9). Although settlement systems can be imagined that generate hierarchies based on the aggregate interactions of individuals seeking to minimize transport costs, what do we learn of ethnological interest from doing this? Particularly, when studying stratified societies, in which individuals have unequal access to strategic resources, it might be the case that all parties are not interested (or able) to minimize transport costs. Or, in egalitarian societies, might it not be the case that the distribution of transport costs is one of the leveling mechanisms—a leveling achieved by unequally assigning higher transport costs to those who begin to develop unequal access to other strategic resources? The suggestion is that alternative ways of thinking about social and spatial relations start from more ethnologically provocative assumptions than those in the social physics of conventional central-place models. Minimally, the societies of these models should incorporate inequalities, and the models should consider how landscapes are created as a result of, and to maintain, these inequalities.

### *Political Economy and Settlement Systems*

One alternative stresses the power relations implicit in settlement systems. Developing a political economic approach to settlements need not abandon all aspects of conventional central-place theory. Useful aspects include the importance of the effect of the friction of distance on social relations, and an expectation for the landscape of capitalist societies.

To briefly elaborate on these points, a key concept offered in geographical models is that space is not socially irrelevant. All too often anthropologists have missed this aspect of space by conceiving of it as a dimension of measurement (e.g., Spaulding 1960), while paying little or no attention to its role as a socially conditioned factor. Specifically, overcoming distance in-

volves a cost, and although this much is true of all societies, the distances overcome and the distribution of costs are socially determined.

The point is most clearly seen in the relationship between surplus production and settlement patterns. Any society produces and distributes a surplus (e.g., Hindess and Hirst 1975; Wolf 1966, 1981). This production involves bringing together people, tools, and natural resources. Accomplishing this production requires, in part, solving locational problems. For instance, are the people who will do the work moved to where the tools and the natural resources are located, or, are tools and natural resources moved to where the labor force is located? Alternatively, are all three moved to new locations for the performance of production? Movement also enters into the distribution side. For instance, are those who moved labor, tools, or natural resources compensated when the production is distributed? If so, at a rate less than, equal to, or greater than the energy they expended?

The production and distribution of surplus obviously involves spatial relations. And just as obviously, there will be physical results of these decisions, results that are recovered as settlement patterns. On the production side, for instance, labor reproduction implies domestic structures, production involves factories—shops—fields, raw materials are extracted in mines and quarries, and transportation uses roads, warehouses, and marketplaces. Relative locations of these fixed structures should be informative about the distribution of costs. For instance, is worker housing near or far from natural resources, or from locations of production? Are production locations easily accessible to many or few consumers? Do transport facilities (e.g., roads, depots, docks) equally link all the factors of production with the locations of production? all the locations of production with all potential locations of consumption?

To summarize, inherent in any production is overcoming the friction of distance. There is, however, no single way to locate fixed structures to overcome this friction. The nature of the solutions depend on the institutions within a society that allocate labor to tools and raw materials, and the institutions that then distribute the surpluses among the sectors of society. As these institutions vary, one would expect the organization of production and the costs of transportation to be differentially arranged. In other words, as modes of production vary, so should associated settlement patterns.

What is the range of variation in settlement patterns associated with varying modes of production? Answering this goes well beyond the scope of this paper. It does suggest a role for central-place theory. Conventional central-place theory assumes that people allocate resources and distribute production through the market. Thus, conventional central-place theory gives insights into the landscapes of capitalism (e.g., Harvey 1973). The

challenge to anthropology is to discover the landscapes associated with alternative modes of production and distribution (e.g., Smith 1976c).

Although stimulating hints concerning an extended theory exist (e.g., Harvey 1973; Peet 1975), no complete alternative models are available. A guiding principle is that cultural landscapes are never neutral; rather they are constructed by members of specific political economies. Although the aims of the various actors may be to perpetuate or to change these relations, the actors are affected by the landscape in which they reside, and their actions regarding social production will in turn transform this landscape. A useful point for understanding how a stratified landscape is transformed is to study the manipulation of the friction of distance in the perpetuation of inequality.

For purposes of illustrating the intertwining of spatial relations and power relations, presume a simple stratified society. I have in mind Fried's (1967:185–242) notion of stratification, namely the creation of inequality of access to strategic resources and the perpetuation of such relations. In this model society, there are elites and nonelites, the former possessing access to strategic resources, such as energy and/or sanctity and/or force, that gives them power over the nonelite (R. N. Adams 1975). The elites are interested in maintaining and possibly increasing the access gap; the nonelite are minimally interested in changing these social relations. The following are a few possible ways in which the friction of distance might be related to these different strategies of reproduction and how they are crystallized as a given settlement pattern.

One way in which elites can reproduce their position is to locate fixed items of the infrastructure (e.g., roads, terminals, government buildings) or to arrange schedules of mobile infrastructure (e.g., subway and bus schedules) to minimize their costs without considering the effect of such locational and scheduling decisions on the costs of nonelites. The twentieth-century United States settlement pattern provides evidence of this reproduction effect, in both the ideology of planning as well as the physical constructions. For example, Morrill and Symons (1977) point out how the models used in making locational decisions in some instances lead to poor people bearing society's transport costs. Specifically, they (1977:224) note that "contrary to the presumption of location theory, an efficient location pattern that maximizes system profits or minimizes system costs, including travel, may result in socially unacceptable inequality in access over space, usually owing to area variations in density and income."

For a specific example, consider a government trying to cut taxes. It might use arguments of spatial economy of scale to centrally locate public services, such as health care at large hospitals, without considering the

implicit spatial costs of accessing these facilities. Morrill and Symons (1977:222) note in this regard:

In the past, it was considered "efficient" to concentrate care of the poor in one large county hospital, often beyond better, but intervening, hospitals that served paying patients; only recently has it been found more efficient, as well as far more equitable, to subsidize the care of the poor at nearby voluntary or private hospitals.

They note that the latter decentralized locational patterns cost more per unit service provided (and are less system efficient), but that the efficiency of centralization is achieved by shifting the burden of accessing these facilities from the society as a whole to the poor. Empirically, this shift toward equitable (rather than efficient) health-care locations can be seen in the proliferation of community health centers in the late 1960s and 1970s. If the tendency to consolidate health care, a trend developing in the 1980s, is realized in the spatial concentration of this care in major hospitals (Berkson 1980; Kleiman 1980), the result will likely be a shift back to a less equitable health-care landscape.

As a second case, elites may seek to maximize the distances among groups of nonelites as a way of increasing the difficulties for organized nonelite group action. An important trend in the United States settlement pattern has been the dispersion of production sites and residences outside of the eighteenth- and nineteenth-century urban centers. David Gordon (1978) points out one factor behind this trend. Conscious locational decisions were made in the late nineteenth century by factory owners to relocate outside of the city as a way to separate workers in different factories. It seems that every time one factory went on strike, the strikers would march down the street attempting to get other workers to go out on sympathy strikes. In the suburbs, factories were separated by enough distance to make sympathy strikes more difficult to organize.

The spatial political economy of capitalism (and other political economies) is not simply one of elites versus nonelites. Locational decisions of infrastructure location may also reflect the competition between elites. Thus, New Haven, Connecticut, and Northampton, Massachusetts, tried to bypass the entrepôt of Hartford in the nineteenth century through canal construction (Martin 1939). Similarly, Utica and Syracuse, New York, managed to bypass Rome, New York, with the Erie Canal (J. Antici, personal communication 1980). These reflect merchants and entrepreneurs competing from these different towns.

Finally, nonelites can use the friction of space to isolate themselves

from more powerful forces. The Long March is only a recent example of the general principle noted by Wolf (1969) that successful peasant wars generally occur if the peasants effectively isolate themselves from continual harassment by the central state.

These observations on the use of space by factions within stratified society are only suggestive of what a fuller integration of political and spatial economy promises. One shortcoming is that they are all drawn from the modern world. They do, however, suggest a way to imaginatively integrate social and spatial surplus accumulation. Their challenge is to fruitfully incorporate the frictional effects of distance in models of premodern stratification. The resultant spatial systems would be far more relevant for evaluating past settlement patterns than the assumptions underlying conventional central-place models. And, using models incorporating spatial inequalities to evaluate past patterns is more likely to lead to interesting ethnological theories of stratified society.

## CONCLUSIONS

The study of the settlement patterns of prehistoric and historical stratified societies has been an important theme in contemporary archaeology. Settlement systems, based on central-place theory, have enriched our understanding of culture process and culture history. However, these models and associated test implications, such as rank-size analysis, have seldom been applied in a critical manner. Problems of bias, boundaries, and behavioral assumptions all deserve closer attention.

A basic strategy for coping with all three problems is to develop alternative models. In the section on biasing processes, the major suggestion is that explicit models of potential biasing processes, as well as the settlement models, are needed if this problem is to be overcome. The section on pooling and peripherality pointed out that generating alternative models of rank-size deviations under varying conditions of pooling is a useful way to sort out the methodological problem of interpreting convexity. And, the last section pointed to the need to identify the landscapes implicit in noncapitalist modes of production to supplement the central-place landscapes associated with capitalism.

That the notion of alternatives emerges from this discussion of settlement studies is quite reasonable given the volume's concern with archaeological methods and theories. The solution is not to throw out frequently used methods, but to place them within the context of alternative approaches, methods, and theoretical insights. Individuals can then choose

definitions, methods, and models that most closely address questions they find interesting. In this richer context, behavioral problem-solving, and not technical virtuosity, can direct archaeological research.

## ACKNOWLEDGMENTS

Thanks to Gregory Johnson, Carol Kramer, and Dolores Root for their comments on earlier versions. Especially thanks to Joan Gero who has the recipe for Powdermilk Biscuits. And thanks to R. Drinkwater and C. J. Masse.

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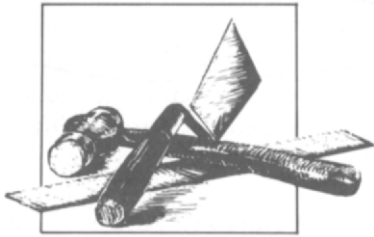
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# 12

## The Social Representation of Space: Dimensioning the Cosmological and the Quotidian

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*Space that has been seized upon by the imagination cannot remain indifferent space subject to the measures and estimates of the surveyor.*

Bachelard 1964:xxxii

The recognition of spatial patterning as one of the most obvious dimensions of patterning in the archaeological record underscores the methodological immediacy of the archaeologist's concern with spatial themes. Yet, as the poet, the philosopher, and the social scientist have argued, the experience of spatial ordering is of critical significance to human perception—sensory, emotional, and conceptual—and to human activity. Thus the archaeologist as humanist and as social theoretician is necessarily interested in the spatial aspects of cultural activity and representation. It is therefore understandable that the theoretical and methodological handling of the theme of the cultural organization of space is well-ensconced in the traditions of the archaeological discipline.

As the preceding quote of Bachelard argues, beyond the concept of objective space that has proved convenient to the arguments of the natural sciences and an empiricist philosophy, a concept of space must be understood within a social and cultural context. Space is a social category defined alternatively within different social and historical contexts. As a social category it is not only necessary to the *prose* of the organization of daily activity, but it is also relevant to the *poetics* of an understanding of the human drama. This use of the term *poetics* is intended to make reference to a conceptual sphere invoked by the original Greek roots of the term: *poiétikos* (inventive; ingenious) and *poiétés* (maker). Given this conceptual sphere the term *poetics*, as employed previously, alludes to the theoretical argument that culture must be understood as meaningfully constituted, where individuals are seen as not only inhabiting, but also creating, the meaningful universe of their existence. This creation of a meaningful context of existence involves, in part, the recognition and the assignation of *order* in society and in the universe. Thus space, as one of the most immediate dimensions of order as perceived and conceived, must play a critical role in the self-definition of society and the meaning it assigns to an order of physical nature.

The increased capacity of certain societies for conceptual (in the sense of the formalization and systematization of representational schemes) and material production entails not only an increasing capacity for spatial organization, but also an increasing capacity for “creating” space. Urban space and public architecture are examples of such created space. Regarded in terms of created space, urban space presents itself as a prime field for the symbolic representation of both the social and the cosmological order. The study of certain facets of urban organization (as a representational field) can provide information on the self-definition of society within various historical contexts. Such study can also help to disclose the relationship that is understood to exist between the social and the cosmological order within such contexts. These themes of societal self-definition and of the relation of the cosmological to the social order within given historical contexts are ultimately relevant to our questions on social formations. They are relevant to our questions on a society’s capacity for self-maintenance, for internal change, and for interaction with and action upon its environment.

## THE HUMAN AND THE SOCIAL QUALITY OF A CONCEPT OF MEANING

Various archaeologists have questioned the validity of a concept of space that is considered an a priori analytical category. They argue, rather,

that space is a socially defined dimension that is experienced and understood within a cultural context. Childe made such a point in his work, *Society and Knowledge*, when he maintained:

Space as a category is not that in which things are perceived [as raw materials of knowledge in a universe of Kantian form], but that in which members of a society co-operate and act together on things (Childe 1956:75).

Childe recognized that the effective theoretical handling of spatial concerns in the archaeological context was one aspect of a more encompassing theoretical argument. This larger argument places theoretical emphasis on the social and human quality of *all* culturally used and produced objects of study confronting the archaeologist. Childe maintained that: "I [as a prehistorian] must treat my objects always and exclusively as concrete expressions and embodiments of human thoughts and ideas—in a word of knowledge" (Childe 1956:1).

To define and justify the archaeological project as a social theoretical project, Childe found it necessary to rework a concept of knowledge. This reworking was necessary to free the concept of knowledge from the constraints placed upon it by the school of logical positivists. The *social* and the *historical*, the critical dimensions of the archaeological object and context, were ignored, if not denied significance, in the positivists' concern for absolute knowledge. It was the recognition and valuation of concepts of society and history, in accordance with Childe's understanding of Marx's thought, that underpinned his definition of knowledge (and rescued his arguments from reaching the same impasse that eventually led to the decline of the positivist school). However, Childe's understanding of society and history in the definition of knowledge and a cultural object of study are open to theoretical questioning. Childe came to narrowly equate the socially significant with the pragmatic and to view history simply as the temporal dimension of error. As a consequence, Childe's arguments leave "meaning" external to a given social and historical context making it the property of an ahistorical objective observer.

At one point in his work Childe stated: "To deserve the name, I contend, knowledge must be communicable and in that sense public and also useful" (Childe 1956:4). The social and the pragmatic were tightly linked in Childe's arguments. According to Childe, the only context and the only means for judgment and correction of error was society (collective social action). It was therefore the only context in which knowledge could be ascertained. Childe rejected a concept of absolute knowledge by defining truth and error in terms of a concept of utility that varied in definition across social contexts. Within the lines of this argument it was a notion of

the pragmatic (refined in his definition of a concept of progress) that provided Childe a theoretical directive in approaching the study of society.

This genre of theoretical argument, “practical theories of culture” as Sahlins (1977:15) has called them, remains *de rigueur* in most archaeological circles. Arguments that focus on the functional, the technological, the ecological, and the economic are eminently convenient to the archaeologist, both methodologically and theoretically speaking. This pragmatic approach defines fixed categories of activity and material context that are quantifiable and potentially recoverable in the archaeological record. More importantly, they guarantee that the inventory of archaeological data overlaps extensively with the domain of cultural activity and products that are seen to carry critical social theoretical relevance. Certainly, “objective indices” (Merleau-Ponty 1974) of the social form are important to understanding the operations of social formations and their changing forms. These include resource potential of the ecological context, volume of material and energy conversion, and organizational potential with respect to surplus production. However, they are not sufficient for the task. They ignore the fact that the control, organization, and transformation of matter, energy, and information are not only effected but also “understood” in a given context of human perception and conception. This is the point made by Merleau-Ponty when he argued that

value, yield, productivity, and maximum population are objects of a type of thinking which *encompasses* the social. We cannot require them to appear in the individual's experience in a pure state. The variables of anthropology, on the contrary, must be met with sooner or later on the level at which phenomena have an immediately human significance. [Merleau-Ponty 1974:116].

What is theoretically at issue both in practical theories of culture and in Childe's definition of knowledge is the problem of meaning, in particular, its locus and its dimensions. If we equate meaning to knowledge and knowledge to the pragmatic then, as Sahlins argues, “The final logic of cultural form is beyond the character and relativity of any human conception” (Sahlins 1977:15). The distinctive quality of the social context loses its theoretical significance in appeals to explanations of general material causality or abstract systemic operations. Childe recognized this latter problem of guaranteeing a *social* quality to his object of study. He sought to give theoretical weight to the social form by arguing that society was the necessary *existential* context of knowledge. He maintained that a concept of the pragmatic could only be defined in terms of “socially approved ends of action” (Childe 1956:112) and that such ends vary historically. However, Childe's conception of history in terms of a progressive accumulation of

knowledge through the successive correction of error lets meaning once again slip from the social context. It does so by admitting no *finality* of meaning within a given social context. Perhaps the following quote of Merleau-Ponty will clarify this last point.

Since we are all hemmed in by history, it is up to us to understand that whatever truth we may have is to be gotten not in spite of but through our historical inherence. . . . As long as I cling to the ideal of an absolute spectator of knowledge with no point of view, I can see my situation as nothing but a source of error. But if I have once recognized that through it I am grafted onto every action and all knowledge which can have meaning for me, and that step by step it contains everything which can *exist* for me, then my contact with the social in the finitude of my situation is revealed to me as the point of origin of all truth. . . . And since we have an idea of truth, since we are in truth and cannot escape it, the only thing left for me to do is to define a truth in the situation [1974:106].

With a theoretical perspective that defines culture as being meaningfully constituted, the present dilemma is that of inserting meaning into the historical and the social context by means of a different appreciation of its human and social dimensions.

## THE PROBLEM OF THE SYMBOLIC IN ARCHAEOLOGY

As argued previously, the concepts of the pragmatic and the adaptive are not sufficient paradigms for understanding culture as meaningfully constituted (Sahlins 1977; Touraine 1977). The human capacity for symbolic creation has served as a major element in the definition of culture. Additionally, Durkheim's argument for the independence of sociological (social scientific) perspective was founded on a notion of representation. By making the symbolic gratuitous to a determinant material order, the possibility of a theoretically useful concept of representation is jeopardized. The individual does not simply inhabit a meaningful world, but is the creator of (in the sense of conferring meaning upon) that world. The problem of the symbolic for the social sciences is not simply the description of symbolic categories, but also the problem of the formation of conceptual categories, their changing content, and their changing structuralization within a larger representational scheme. Such a representational scheme not only provides the categories and organization necessary to the routine of daily activity, but also confers meaning and coherency on such activity by relating it to the more encompassing concerns of the perceived and conceived structuring of society and the order of physical nature.

Consequently, simply increasing archaeology's sophistication in the *identification* and *description* of symbolic matters by means of formal analyses is not sufficient to the handling of the theoretical problem of the symbolic in archaeology. Part of the theoretical task facing the archaeologist as social scientist is the revaluation and exploration of a concept of social representation as it relates to an order of activity. Though this project is beyond the scope of this chapter a few lines of thought on this concern might be fruitfully explored in an attempt to approach the problem of the symbolic use of space in social theory.

The most obvious challenge of the theoretical handling of a concept of social representation is avoidance of both a subjective idealism and a psychological reductionism. This means that in giving theoretical credence to a concept of representation, it is not simply enough to refer to conscious and volitional individuals. It is also necessary to understand that the actions of such individuals have material consequences. Those consequences are necessary to the interpretation of such actions and create a continuing context for individual experience and expression. In this regard, Marx's historical materialism can be viewed as a revolutionary theoretical step when understood as an attempt to integrate individual praxis with structural and material causation (Brown 1978:16). Marx's work is predicated on notions of subjectivity and the individual consciousness. It was the immediate focus of his earliest works, and concepts of freedom and consciousness are essential to an appreciation of Marx's continuing discourse because they underpin his characterization of society. It was the loss of awareness by social beings of their creative role in the construction of a social order that provided Marx with his later problem orientation. Though Marx's later works focused attention at the level of the social formation, his concern for economic organization and material context did not prevent him from recognizing the conscious individual as problematic in social analysis.

In studying [social] transformation it is always necessary to distinguish between the material transformation of the economic conditions of production, which can be determined with the precision of natural sciences, and the legal, political, religious, artistic or philosophic—in short, ideological forms in which men become conscious of conflict and fight it out [Marx 1970].

Marx can and has been quoted as the Bible. Yet, Marx's elaboration of such concepts as alienation, reification, and false consciousness testify to his continuing sensitivity to the problem of the individual's consciousness in social theory (Brown 1978:17). This contradicts the conclusions one might draw from works that cite his name to certify simplistic materialist and economic determinist interpretations of society and culture.

Marx has provided us with a valuable sense of the problem (*problématique*) in approaching the issues of consciousness, individual, and society in social theory. Yet, the resolution that his work suggests for these issues is problematic. Williams's assessment of Marx's handling of these issues is insightful.

Consciousness is seen from the beginning as part of the human material social process, and its products in "ideas" are then as much part of this process as material products themselves. This, centrally, was the thrust of Marx's whole argument, but the point was lost, in this crucial area, by a temporary surrender to the cynicism of "practical men" and, even more, to the abstract empiricism of a version of "natural science" (Williams 1977:59–60).

It might also be noted here that this remark of Williams allows us to understand Childe's reading of Marx as one possible interpretation, rather than as a misreading, of Marx's thought.

Today it is still a pragmatic problem focus and a classical empiricism that plague the social theory of archaeologists and most often orient their Marxist analyses. The major difficulty with such traditional applications of Marxist thought to social analysis is that they impose a rigid dichotomy on matter and symbol and thus on activity and representation. This results in the theoretical move of assigning matter and activity to a "determinant" infrastructure or base, and symbol and representation to a "determined" superstructure. The problem of representation thus appears to be theoretically diffused in a classical understanding of the concept of ideology.

To separate activity from representation is only possible if we view our social object of study as culture, reified as system, with simple reproduction and adaptation as its processual concerns. However, if we separate activity from representation, then we misunderstand the nature of symbolic activity in a social structure. We must not separate the question of *how* a commoner labor force produces a material surplus and a monarchy functions as an organizing administrative and information processing subsystem from the question of *why* commoners and *why* kings. We must not continue to reify society as actor or as system. Rather, society must be understood, as Marx urged, as individuals engaged in creative social action. If human activity is by definition symbolic, then social organization cannot exist separate from its representation (Agué 1975:xix).

The theoretical position of the sociologist, Touraine, provides a valuable alternative to that of accepting the rigid dichotomy of activity and representation imposed by a strict materialist–empiricist definition of society. Central to Touraine's argument is an insistence on the interrelationship of activity and representation. He argues that the symbolic capacity of

society allows it to posit, besides an *order of activity*, an *order of representation*. These are not to be understood as two separate orders, one of material situation and one of idea. Rather, it is characteristic of social action to constitute its experience through the *meaning* it gives this experience (Touraine 1977), thus inextricably uniting activity and representation. For Touraine, "society is not just reproduction and adaptation; it is also creation, self-production" (Touraine 1977:3). An order of representation permits society to *distance* itself from its activity and as a consequence, society cannot be adequately defined by the state of its productive forces. Society has the capacity to define itself, to transform its relations with its environment and to constitute its milieu or context of meaning, and thus to self-create (Touraine 1977:3-4).

Touraine has applied the term *historicity* to the "distance society places between itself and its activity" and the "action by which it determines the categories of its practice" (Touraine 1977:4, 461). Touraine understands the concept of historicity as a combination of three components: (1) accumulation; (2) a model of knowledge; and (3) a cultural model. Accumulation refers to a society's surplus productivity, a model of knowledge to an image of society and nature, and a cultural model to the image society has of its capacity to act upon itself. It would be a grave mistake to view Touraine's components of historicity as rigid institutional distinctions or in terms of contemporary cultural categories. That is, we would miss the point of Touraine's understanding of the dynamics of society's self-production and of social representation if we were to view a model of knowledge as simply economics and scientific knowledge or to view a cultural model as a dominant ideology. Even to view accumulation as simply a question of technology and man-power is to ignore the fact that a concept of surplus and accumulation vary in designation and meaning across cultural contexts. The accumulation and subsequent distribution and consumption of surplus by priests, by kings, or by workers is neither equivalent in representation nor in structure-function.

To approach the problem of social representation and a concept of historicity as they relate to a society's intellectual and material capacity for action upon itself and its physical environment, it is perhaps helpful to temporarily forsake traditional social institutional categories such as religion, politics, and economics. Such categories serve to reify society as system and to focus our attention above the level of the individual. We might better restore a human scale to our questions about social formations by attempting to understand the more general common denominators of representational orders. With such a perspective we might be better able to pose questions on the formation of social categories with the hope of understanding social activity, material production, and social institutions as as-

pects of social creation within a meaningful context. This is a context that includes not only the limitations of the structure of the physical universe and the operational logic of systems, but also the creative, productive, and organizational potential of social representation. This point has been made more elegantly by Merleau-Ponty in his reflections on the sociological understanding.

Sociology [and social science in general] should not seek an explanation of the religious in the social (or indeed, of the social in the religious) but must consider them as two aspects of the real and fantastic human bond as it has been worked out by the civilization under consideration and try to objectify the solution which that civilization invents in its religion as in its economy or in its politics to the problem of man's relation with nature and with other men [1974:165].

To restate the previous arguments, if we are to do justice to a concept of representation and to understand society as self-producing, rather than as simply reproducing, then we must not stop with the description of the material, productive, and abstract organizational character of a given social formation. We must also explore the concepts and conceptual structurings critical to the representation of that social formation. That is, we must explore the ordering principles employed by individuals within alternative social contexts to define society and its categories of activities and relate these to its physical and historical context. We will have to explore a set of emergent concepts and their changing historical content and articulation as they relate to the formulations of and the solutions to the enduring questions of the relations between individual, society, and nature. These will often be concepts that a simple behavioral psychology and a classical philosophy cannot adequately specify within alternative historical contexts, concepts such as person, authority, power, history, order, creation, social hierarchy, and society. If we recognize that such concepts and their conceptual structurings will necessarily find expression in activity, in institutional categories, and in material symbols, then their exploration is open, in part, to the archaeologist.

## CREATED SPACE

Time and space are the dimensions of our most immediate experience of order as lived and as thought. If we consider a notion of representation in terms of a structuring and an ordering, then time and space should play critical roles. Those roles include not only the organization of daily activity, but also the definition and representation of concepts of individual, society, and nature, as well as the mapping of the relations understood to exist

between them. This is to say, the human use of space and time can not only order the quotidian but also map the cosmological.

Fletcher's work on settlement studies is an example of an archaeological study that considers space in the cultural context, not as a simple analytic category, but as a meaningful dimension necessary to the social experience. In his attempt to carry considerations of the cultural organization of space in archaeological studies beyond those addressed within a functionalist perspective, Fletcher argues that

the human use of space is not only directed by immediate and material, functional or environmental controls, but it is also patterned by the human brain's need for signals specifying the similarities or differences between various parts of its context and by the use of classifications of space as an adaptive mechanism for coping with the environment [1977:48].

Such an argument recognizes that the context of culture includes not only the physical-ecological environment, but also the social context as well, a context of "different categories of activity and . . . people of different social status" (Fletcher 1977:49). Fletcher contends that archaeological studies of spatial concerns must complement the current *functional* analyses with *formal* analyses—descriptive studies of visual and conceptual pattern in reference to systems of classificatory relationships. Fletcher's point is a valid one. However, as argued in the previous section, the recognition and description of socially defined categories still leaves aside the question of the formation and the articulation of these categories within a more encompassing sphere of social experience and representation wherein their meaning is specified. This is to say, Fletcher is correct in arguing for considerations of the *human dimension* in spatial analyses, recognizing that it is the individuals in a given social formation that have their feet planted on the ground. Yet, we must at some point recognize as well that it is these same individuals who often have their heads in the stars or vice versa.

It was Eliade who remarked

all territory occupied with the objective of being inhabited or of being utilized as "vital space" is necessarily transformed from "chaos" into "cosmos"; that is . . . such space is given form [my translation, Eliade 1949:29].

To toss off Eliade's poetic and evocative image of "chaos" and "cosmos" as simply an inspired romanticism would be to overlook the deeper insight of this remark: it understands the cosmological and quotidian as interwoven in both thought and practice. In an order of representation, this articulation of the cosmological and the quotidian is *not only possible but necessary*.

Geertz has made an interesting remark on the role of religious symbols in the articulation of these two domains:

Religious symbols formulate a basic congruence between a particular style of life and a specific (if, most often implicit) metaphysics, and in so doing sustain each with the borrowed authority of the other [1973:90].

This remark of Geertz can be generalized and extended to the larger field of representation. In essence, both the self-evidence of the lived order and the appeal to the authority of a higher order are necessary to the definition, legitimation, and maintenance of a social order that does *not* recognize itself as resting upon nothing other than individuals engaged in creative social practice. The social ordering and representation of space should in some way reflect this consideration.

Spatial ordering permits a certain degree of human choice (e.g., site placement, boundary demarcation), manipulation (e.g., symbolic investment, mapping), and creation (e.g., architecture, urban planning) as well as destruction. The geographer Harvey has spoken of a distinction between *effective space* and *created space* in the cultural use of space. Effective space makes reference to “subtle symbiotic interrelationships between social activities and organic nature” (Harvey 1973:309), whereas urban space, as a “giant man-made resource system” (Harvey 1973:309), is given as the prime example of created space. This distinction between effective space and created space, however, is not as straightforward as Harvey would have it. All space as put to human use is created in so far as it is invested with form and meaning.

Harvey introduced the distinction of effective and created space in arguing that, with increasing differentiation in social formations “*created space* replaces *effective space* as the overriding principle of geographical organization” (Harvey 1973:309). As a generalized argument the point is valid. However, Harvey has gone on to argue that

in preindustrial society natural differentials in resource availability and in natural environments formed the basis for geographical differentiation. Effective space was created out of ecological differentiation by arranging for the flow of goods and services from areas of supply to areas of demand [Harvey 1973:309].

A different light is shed on geographical organization in preindustrial societies by various studies of cultural systems wherein levels of sociopolitical organization extend across local subsistence groupings. The case of precontact Hawaii is an example.

It has been shown for Hawaii that redistribution of most material goods by the chief was not the major factor in the union of independent communities in diverse biotic and physiographic zones (Earle and Erickson 1977; Peebles and Kus 1977). The basic geographic unit of production and consumption in precontact Hawaii was designed to contain within its boundaries sufficient environmental diversity and productivity to guarantee self-sufficiency with regard to subsistence needs. Rather, it was sumptuary items designated for the exclusive use of the chief and his retinue that were differentially distributed in this form of geographical divisioning and interdependence. As is demonstrated in the Hawaiian case, the question is not manifest regarding the formative role of natural environmental differentials in preindustrial "complex" social formations as it relates to concepts of effective and created space. "Complex" social formations can choose to inaccentuate as well as to emphasize ecological diversity in the creation of social boundaries and territorial divisions. There is also the question of subsistence and sumptuary items as socially designated categories. There is also the problem of the differential distribution of resources that include not only agriculturally productive land, but also symbolically invested features of the landscape that are important to the self-definition of a social formation. However, there is an insight to be gained from the recognition of the increasing ability of societies to manipulate and to create, in a material sense, the space they inhabit as we move from one end of a continuum of social organization to the other. This problem of created space becomes most interesting when focused on towns and urban centers where a concept of effective space is particularly insufficient to explain the form of internal organization such centers of social activity take (Harvey 1973:309).

Whether we speak of chiefdoms and states or of complex societies, or whether we employ an alternative categorical designation, it is the case that in certain social contexts there exists a marked potential for the materialization of social products. This potential includes the possibility of public and monumental architecture, and the construction of towns and cities. This material potential is accompanied by, if it does not necessitate, a certain level of social discourse (Bourdieu 1977:233) or intellectual specialization (Marx and Engels 1970). Such specialization permits a high degree of formalization, systematization, and rationalization of an order of representation. This consideration should have some implications for the legibility of a representational order in the material creations of a society. In such social contexts it is perhaps the larger towns and urban centers—the physical locus of individuals designated as authoritative representatives of the social order, of administrative activities, and of social institutions—that present the material domain *par excellence* for the mapping of conceived representational schemes of the social order and its relation to the cosmos. The significant

degree of urban planning characteristic of early “states” would seem to support this general direction of inquiry.

## THE URBAN CENTER AS REPRESENTATIONAL FIELD

In his work, *The City in History*, Mumford contends that “the city was a new symbolic world, representing not only a people, but a whole cosmos and its gods” (Mumford 1961:36). The archaeologist has often resorted to simple caricatures of theocracy, divine monarchy, and merchant-state to categorize the alternative symbolic worlds of early cities. More often than not, these categories are employed as a veneer to give historical content and form to what is seen to be the more important underlying economic question of redistributive and market systems. In such a perspective, ideological concerns are viewed as the administrative overhead and symbolic format as the code for information processing. Both are seen to be necessary for the functioning of a system with a less-than-perfect understanding of its operational logic. The organizational *functions* of symbolic activity are assigned a theoretical role, and the specific symbolic *content* of such activity is left as superfluous to abstract systems operations. Yet, it is precisely this theoretical separation of organization and activity from representation and meaning that was abjured in the preceding discussion.

The explanation of the urban form is a complex question that goes beyond abstract functional and economic considerations. Authors such as Mumford, Adams, and Wheatley have addressed such an issue in their works, works that attempt to appreciate the urban form beyond its skeletal systemic form. Mumford, for instance, has argued that the concrete urban *feature* of city walls, a feature easily equated with defensive purposes, may have first served as a feature within a religious context “to defend the sacred limits of the *temenos*” (Mumford 1961:36). An early work of Adams on the urban *process* argues that

The rise of cities . . . was pre-eminently a social process, an expression more of changes in man's interaction with his fellows than in his interaction with his environment [1960:3].

Regarding the heavy symbolic investment witnessed in the *form* that the construction and layout of early urban centers follow, Wheatley has argued for the case of Asian capitals that

[these] capitals were not, as is sometimes supposed, solely—or even primarily—expressions of pomp and glory, though these considerations did enter into their

construction. Rather, they were the material instruments of a particular political theory, and the symbolism inseparable from that role was not a mere decorative veneer but one of a functionally interrelated core of urban institutions [1969:18].

Harvey has justly criticized Wheatley's and Adams's larger theoretical arguments on urban origins and development which give primacy to social institutional forms. He argues that they see "social change [as] . . . attributed to a moving force in the minds of men rather than to a necessary evolution of social practice" (Harvey 1973:221). Yet, Harvey does not propose a simple materialist-economic explanation of urban origins and form. He admits, rather, that

initially [the city] functioned as a political, ideological and military force to sustain a particular pattern in the social relations of production. . . . Many of the functions of the city during this period have to be categorized as superstructural. . . . To say that the function of urbanism was to fashion a superstructure in support of a particular patterning of the social relations of production is not to say that urbanism was a mere product of forces in the economic basis of society [1973:304–305].

Within a traditional material-economic perspective the reflections of Adams, Wheatley, and Harvey certainly encourage one to view the question of the material reproduction of society as displaced from the locus of early urban centers. However, within a perspective that recognizes a concept of social representation as critical to an understanding of social formations, they encourage one to view the early urban center as a representational field wherein the representation of individual as labor value, of society as material production and consumption, and nature as exploitative potential measured in terms of labor inputs and capital gains is neither valid nor meaningful. The exploration of the urban form as a representational field can offer insights on differing formulations and solutions that have been offered to the question of the relationships among individuals and between the social order and the cosmos.

## **SOCIETY, COSMOS, AND THE PROBLEM OF LEGITIMATION**

Let us briefly examine certain physical features and spatial arrangements of various urban forms in an attempt to explore the suggestion of Eliade and Mumford that the urban form is a prime field for the representation of the social order and the cosmos as understood within a given social and historical context. As argued previously, a notion of *order* is a critical

element in the definition and understanding of the social and the cosmological. Eliade, for instance, has spoken of the importance of a notion of a center or *axis mundi* in the establishment and subsequent spatial organization of certain habitation sites. A central location has often served as the key element of orientation with respect to locating ordering forces and mapping the established social order. The individual or institution that occupies the central location within a town or an urban center that employs such a schema of spatial mapping might therefore offer some insight regarding the nature of the social order as understood within a given cultural context.

Wheatley, in pursuing this idea of an *axis mundi*, has noted that

in the cities of the ancient Middle East, south India, Ceylon and in those of Hindu and Māhāyana South-East Asia, it was a temple which occupied the most sacred site at the axis of the kingdom. In China, by contrast . . . the centrally situated temple of the archetypal South and South-East Asian city was replaced . . . by the seat of secular authority [1969:12–13].

These differing spatial layouts bring to the fore the theme of the sacred and the secular (profane), a classical theme of studies on the nature of power, authority, and order in the social world. Within these thematic bounds also lies the question of the *legitimation* of a social order.

Very generally speaking, the legitimacy of sacred authority is often guaranteed within the ahistoric inevitability of an encompassing natural (physical nature) or cosmological order. Such authority thus guarantees its legitimacy in an appeal to an ordering principle that does not admit of any alternatives and thus does not permit questioning. The legitimacy of secular authority is more problematic.

In the legitimation of secular authority, appeal must be made, in part, to the prerogatives of the social domain. Such prerogatives must in some sense include those issues that are denied in an ahistoric cosmological order, such issues as temporality and change. Considerations of temporality raise the problem of a concept of history and the differing manners in which such a concept can be understood. One possibility is to view the historical past as a model for the social present. When history is considered normative then conformity with its precepts requires no justification (Wheatley 1969:11). However, it is innovation and creation that still remain problematic. There is an alternative to the constraint of historical pattern and its exegesis, an alternative that Eliade has pointed out. This alternative is to “regenerate time” by the valuation of the future (Eliade 1949:157), by the creation of an ideal, and by the recognition of change as necessary to the temporal struggle for its realization.

The nature of the individuals or institutions that occupy a central loca-

tion within an urban context might offer insights on the broad outlines of the problem of the sacred and the secular in the definition of authority and legitimacy in different historical contexts. Yet, it may also be possible to further discern and elaborate aspects of such alternative representational schemes in the material symbols and the spatial mappings of urban centers. For example, in the case of a strong adherence to a sacred ordering principle, this might involve the symbolic mapping of the urban space in conformity with a scheme of natural–ecological balance, a scheme of calendrical divisioning, or a mapping of a dynamic equilibrium of primal forces or elements. In the case of historical models for the social present, this might be reflected in special activity areas within the urban center. These models might focus on historically invested items, individuals (e.g., tombs), and locations. Regarding the cultural context wherein a dynamic concept of history has been elaborated, the case of Imerina in central Madagascar provides an interesting example. It is worthwhile to examine the case of central Madagascar in more detail, because it illustrates the complexity of conceptual concerns that are at issue, concerns that are not often adequately appreciated in our generalized models of dominant ideologies in preindustrial, non-Western societies. Further, the case of central Madagascar illustrates the complexity of the problem of the articulation of the social and the cosmological in a context where the legitimacy of a sociopolitical order makes overt appeal to the prerogatives of a social order.

Aspects of the spatial layout of the capitals of the Merina kingdom of the eighteenth century A.D. (see Figure 12.1) reflect a representational concern with historical movement toward the actualization of a *sociopolitical ideal*. It was the sociopolitical ideal of the unification of the Merina kingdom that served as the point of orientation in the Merina conceptualization of history. The union of the four cardinal directions by a central point (sometimes referred to in its more *abstract* form of the unification of four corners by their center) is the spatially translatable symbolization of this ideal of unification. The capital of Ambohimanga was rebuilt in the eighteenth century A.D. using this plan of the four cardinal directions and the center. More specifically, the royal residence stood at the center of this spatial plan and the seven major gates of the capital were each designated with reference to one of the four cardinal directions. It is interesting to note that, according to oral tradition, the task of rebuilding the gates of Ambohimanga was assigned, not specifically to the inhabitants of Ambohimanga, but to representatives of the whole of the Merina population. These representatives of the Merina population were designated as representatives, not of named kinship groups, but of the four original geographic districts of the kingdom (Kus 1979). The spatial organization of Ambohimanga thus represented not only the abstract geometric expression of

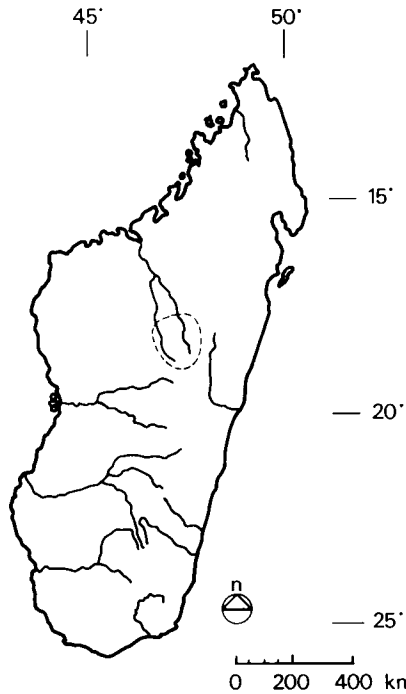
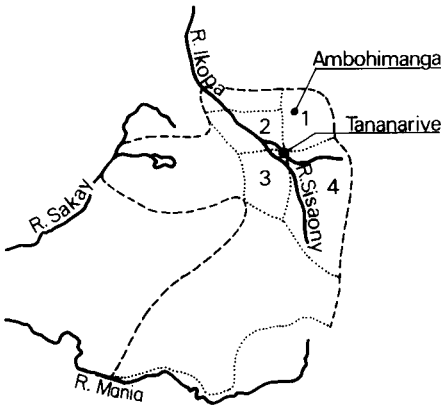


FIGURE 12.1 Location of the capitals of the Merina Kingdom, eighteenth century A.D.

an ideal of political unity but also corresponded to the actual sociopolitical mapping of the original Merina heartland (see Figure 12.2).

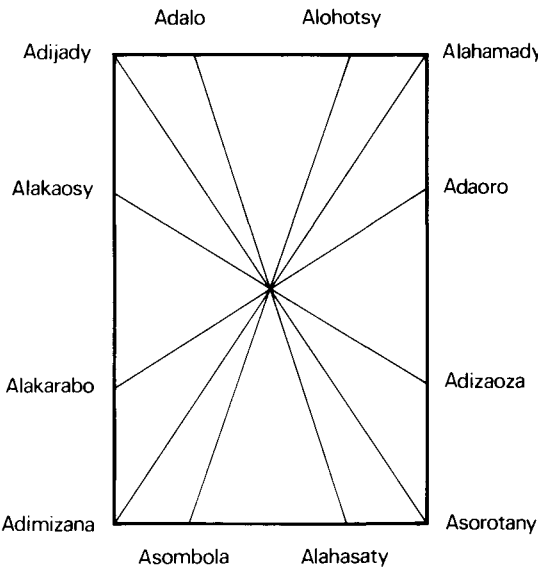
It would be convenient if the representational schemes of the various means of sociopolitical legitimation were conceptually straightforward and mutually exclusive as they may have appeared in their preceding presentation. However, if we look closer at the example this does not appear to be the case. The reconstruction of the Merina capital of Ambohimanga was more complexly organized than previously implied. In addition to the spatial layout of this capital in accordance with the scheme of four cardinal directions united by their center, Ambohimanga was subject to a second and contemporaneous mapping. This second mapping was in accordance with a cosmological scheme of *vintana* or destiny based on a 12-part divisioning of the year (see Figure 12.3). This temporal divisioning was spatially translatable onto a square. The four corners of the square were each occupied by one of the major destinies and on each side of the square were located two points representing minor destinies. For this second mapping it was the village of Ambohimanga, representing the undifferentiated social body



**FIGURE 12.2** Spatial layout of the capital including the four original districts of Imerina: (1) Avaradrano, (2) Marovatana, (3) Ambodirano, (4) Vakinisisaony.

(rather than the royal residence), that served as central point of orientation. Each of the seven main gates of the capital were assigned a second locational reference with respect to this order of *vintana*.

There is something to be learned in the complexity engendered by these overlapping spatial mappings of the capital of Ambohimanga. In particular,



**FIGURE 12.3** Spatial layout of the capital in accordance with the cosmological scheme of *vintana*.

it is the articulation of these two spatial mappings that reveals Merina sociopolitical conceptualizations. The royal residence, center of the sociopolitical ordering, stood to the northeast of the center of the cosmological mapping. It is the northeast direction of the scheme of *vintana* that is occupied by the first destiny of *Alahamady*. *Alahamady* is the most noble of the 12 destinities for it is the position "that accords dignity and sovereignty and thanks to which all can be undertaken" (my translation, Callet 1974:54). Thus it would appear that in Merina social representation, the king and the political order were articulated with the cosmological order by occupying the most privileged and potentially *transcendent* position in that second order.

It is clear that to pursue the line of argument briefly sketched here, it will be necessary both to refine the argument within a field of social and political theory and to examine additional information from alternative ethnographic and archaeological contexts. Yet, despite the preliminary and tentative nature of the discussion on legitimacy and the social order presented previously, the point of the larger argument should not be missed. The point of this argument is that there is a critical relationship between a society's order of activity and its order of representation that is theoretically important to our understanding of culture as meaningfully constituted.

Referring to the specific arguments presented in this chapter, concepts such as history and the legitimacy of a social order, their conceptual emergence and their differing content across cultural contexts are theoretically necessary to our understanding of a society's capacity for self-definition and self-production. These issues are relative to Touraine's understanding of a concept of historicity and, in particular, to his notion of a cultural model "which apprehends and interprets society's capacity for action upon itself" (Touraine 1977:461). A social order whose existence is seen to be dependent upon a metasocial warrant "a cultural model which subordinates social action and analysis to laws that transcend them" (Touraine 1977:462)—is limited in its capacity for creative social change. A social order that confounds its principles of organization and operation and its representation with an ahistoric natural order grants itself a self-evident legitimacy. Yet, it potentially pays a price for its inviolateness in the form of a rigidity that does not admit of the manipulation of the social order save by the artifice of the exegetical reinterpretation of sacred dogma or cosmological intervention. A society that defines itself in terms of prerogatives characteristic of a social order in partial distinction from a natural-cosmological order confers upon itself a greater manipulability and creative capacity to intervene in its own operations. The case of Imerina in Madagascar is an example of such a society. The king, as representative of the social body, was not only accorded, but expected to exercise, the prerogatives of social

innovation and creation. The Merina oral traditions record the various innovations (technological, social, political, etc.) of successive rulers. These innovations were interpreted by the Merina as *progressive* steps necessary to the actualization of a sociopolitical ideal of unity and stability. If a society accepts that it has no recourse to metasocial warrants to explain and justify its activities, that is, if it accepts that the responsibility for the social form rests upon nothing other than the shoulders of individuals engaged in social action (society as understood by Marx and Touraine, among others), then it possesses a profound capacity to creatively act upon itself. This is a capacity yet to be fully explored in contemporary society.

## CONCLUDING REMARKS

The work of Marx was a concerted attempt to provide us with an understanding of society as the creation of individuals engaged in social practice. The thought of Marx has also provided us with a valuable problem orientation in social theory. This question asks how individuals within the context of various social formations come to view themselves as alienated from the products of their praxis and to seek the explanation and justification of the form and operations of society beyond the existential context of social praxis. If "social being determines consciousness" as Marx has argued, then the study of alternative contexts of social experience should provide us with some clues to the understanding of this problem. As Merleau-Ponty has argued

the spirit of society is realized, transmitted, and perceived through the cultural objects which it bestows upon itself and in the midst of which it lives. It is there that the deposit of its practical categories is built up and these categories in turn suggest a way of being and thinking to men [1974:180].

These products of society must be seen to include symbols as well as the material products of subsistence activities and representational schemes as well as social institutions. If we understand that these products of society are produced and conceived, and thus articulated, within a context of meaningful human practice, then the exploration of this social theoretical problem is not only open to archaeological investigation, but is also important to the social theoretical concerns of the archaeologist interested in the study of culture.

It was Schmidt, reflecting upon the thoughts of Marx, who said

He who separates thought from the senses, the soul from the body, is incapable of grasping the connection between the content of culture and the sphere of material production [1971:21].

Archaeologists as social theoreticians confronted with the material products of society must concern themselves with the adequacy of their theoretical understanding of the relation of social activity to representation if they are to understand culture as meaningfully constituted.

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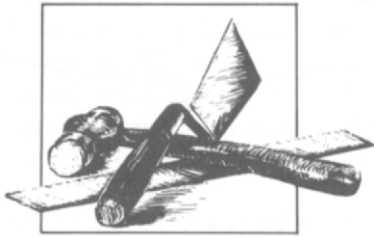
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# 13

## The Poverty of Philosophy in Archaeology

DEAN J. SAITTA

Since the late 1960s traditional archaeological theory and method have come under increasingly critical examination. Not surprisingly, these years have also seen a sharpening of interest in the philosophy of science. Recently, Schiffer (1981) has appealed for intensification along this front, calling for development of a more explicit "philosophy of archaeology." In what he views as an initial step in this direction, Schiffer identifies two issues within archaeology warranting philosophical treatment from archaeologists and philosophers of science alike. One of these concerns the borrowing of theoretical concepts from other disciplines (specifically ecology) to apply to human behavior; the other concerns the problem of confirmation in ethnoarchaeology. The present amount of controversy surrounding these issues suggests to Schiffer the need for a set of philosophically derived procedures to ensure more successful borrowing on the one hand, and the delivery of more universally acceptable knowledge-claims on the other.

Schiffer's specification of these particular philosophical issues suggests an epistemological outlook that is essentially empiricist in scope. The pur-

pose of this chapter is twofold: (1) to explore the consequences of an empiricist epistemology for both archaeology and society; and (2) to suggest an alternative set of philosophical issues that might serve to focus a philosophy of archaeology. The critique of empiricism and the alternative philosophical orientation outlined in this chapter depend upon a particular understanding of what theory is.

## EPISTEMOLOGY, ARCHAEOLOGY, AND SOCIETY

### Theory and Reality

At the basis of empiricist epistemology is the notion that “thought” and “reality” can be treated as independently existing realms of life. The empiricist presumes that the “truth” of reality is contained within the givens of experience in the guise of “facts.” For the empiricist the goal of scientific inquiry is to “mirror” (Rorty 1979) this truth by representing assembled facts as accurately as possible in theory. Achievement of this goal in turn requires a certain amount of fundamental thinking among those seeking to participate in the process of theory building. Since humans are presumed to have privileged access to truth, the empiricist deems it necessary for everyone to agree on what counts as a conceptually relevant way of arriving at truth, so that alternative knowledge-claims can be evaluated. Adherence to this framework thus implicates a view of theory building as an ethically neutral enterprise that is unaffected by other political, economic, or cultural processes operating in society. Schiffer’s call for a set of procedures to mediate theory building, as well as for some guidelines for confirming knowledge-claims, clearly reflects an allegiance to empiricist epistemology.

To the extent that one endorses this epistemological perspective, there is little fault to be found with Schiffer’s arguments. The issues of philosophic import he identifies and the strategies he suggests for resolving them are consistent with his particular view of theory and the purpose of scientific inquiry. From the epistemological position taken in this chapter, however, a philosophy of archaeology that is grounded in empiricism has unfortunate consequences for both the movement of the discipline and for society. This alternative epistemological position is informed by a particular understanding of dialectical materialism (Resnick and Wolff 1982).

Like empiricism, dialectical materialism presupposes a certain relationship between thought and reality. Dialectical materialism differs, however, in viewing thought as but one constituent aspect of a *singular* social totality. Embedded in this social totality are all manner of other political, economic, and cultural processes in society, each of which is presumed to be in

cause–effect interplay with all the others. Thus, this epistemological position denies that the thinking process can ever be a pure or unfettered act of freedom. Rather, the birth, development, and death of any theory is seen to be conditioned by the myriad social processes comprising the social totality. In short, theory is viewed first and foremost as a form of *social* production—as a motive force shaped by society and in turn influencing society in particular directions (the “materialist” thesis). As characterized here, then, dialectical materialism refers only to a particular theory of knowledge; it is *not*, as some would have it, a “political strategy dedicated to the destruction of capitalism and the birth of communism” (Harris 1979:155).

Following from this particular conception of the relationship between thought and reality is the dialectical materialist assertion that *no* theoretical framework (regardless of its epistemological underpinnings) can deliver the “essence” or “truth” of reality. The empiricist claim that such truth can be known via programmatic adherence to a set of procedures for “mirroring” reality is thus rejected. Alternatively, from the standpoint of dialectical materialism the knowledge-claims delivered by *any* theory (including those informed by a dialectical materialist epistemology) can only be considered “true” insofar as their own validity criteria are concerned; that is, they are relative truths. But by rejecting the notion of absolute truth and the idea that theories can be evaluated and rank ordered using some standard measure of validity, the dialectical materialist does not mean to imply a relativist indifference toward alternative theoretical or epistemological frameworks. Rather, dialectical materialism specifies a form of critique that is distinctly nontraditional. Such a critique seeks to (1) expose the social conditions that allow competing theoretical frameworks to exist and (2) determine the consequences of competing theoretical approaches for those other aspects of the social totality with which they articulate. Since, from the standpoint of dialectical materialism, theories end only when their social conditions of existence end, pursuit of these tasks is seen to serve the purposes of radical social criticism.

### The Methodological Beast

A full-blown critique of the empiricist epistemological position is well outside the scope of this chapter. Arguments concerning the drawing of the thought–reality dichotomy, the logic of empiricist proof, and the notion of value-free science are more ably presented elsewhere (Fay 1975; Harvey 1973; Resnick and Wolff 1982; Rorty 1979). The major intention here is only to point out some consequences of empiricist epistemology for both the direction of the discipline and society.

Perhaps the most apparent effect of the empiricist project in any discipline is the disproportionate amount of concern it generates for methodology. Indeed, a tendency for sets of analytical methods to become objects of analysis in their own right seems to be inherent in the empiricist framework (Mills 1959:51). Such a tendency is clearly evident within archaeology today. The unleashing of the “methodological beast” with the advent of the so-called “new” archaeology in the 1960s has created such an interest in mirroring (i.e., increasing the precision of those methods used to recreate reality) that problem definition has become a secondary concern in some areas (see Moore and Keene, Wobst, Keene, and Cross, this volume). Even those archaeologists who have furnished insights into the disciplinary trends of the past 20 years tend to view an appropriate question as one that can be answered with available data (e.g., Meltzer 1979). This is tantamount to saying that the questions archaeologists ask should be defined on the basis of how well they are currently able to mirror reality. It is precisely this kind of neglect for what it is we *want* to know that has perhaps contributed to the perpetuation of “normal” science (Kuhn 1970) in the recent history of the discipline, and the suppression of genuine theoretical revolution. But even normal science depends upon a background of consensus so that agreement can be reached on what qualifies as “truth.” As Schiffer points out, such consensus has not yet emerged in archaeology. This situation attests to the inherent difficulty of arriving at standard criteria for adjudicating knowledge-claims even *within* the empiricist tradition, and thus to the need for some alternative (but distinctly nontraditional) evaluative framework.

The consequences of empiricist epistemology are as profound for society as they are for the discipline. Though the empiricist seeks a neutral framework to direct theory building and “govern the successful transfer of principles from one science to another” (Schiffer 1981:906), even the most “successful” of transfers in these terms is likely to have anything but neutral effects on society. Arguing from the standpoint of dialectical materialism, Harvey (1973, 1974) has demonstrated how theories marshalled by empiricists for application in the real world have predictable results for social policy making. Because such theories are necessarily grounded in the reality they seek to mirror, they usually tend to maintain the societal status quo when practically applied. As an example, Harvey (1974) shows how the empiricist tradition consistently produces theories that sanction Malthusian and neo-Malthusian conclusions concerning the relationship between population and resources. These theories are in turn shown to have particular political and theoretical consequences: they implicitly justify inaction where problems of economic repression are concerned, and effectively divert attention from other kinds of social processes (e.g., class relations) that condition the population–resources relationship. The potentially severe implications

of empiricist theories (a severity enhanced by the “universal truth” status accorded their propositions) thus suggests to Harvey the need for investigating their *social* conditions of existence. This need seems all the more compelling today given the variety of contradictory “facts” that exist concerning the population–resources relationship (e.g., Simon 1980).

In light of Harvey’s argument, then, it becomes conceivable that the empiricist project operating in archaeology can have similar societal-wide effects. Because of the logic of empiricist epistemology, theories rising on empiricist foundations potentially serve only to recreate in the past the dominant cultural ideologies of the present. The continuing popularity within the discipline of population–resource stress models for explaining aspects of social evolution may be illustrative of this situation. When viewed in a social context, such theories serve to implicitly legitimize prevailing socioeconomic conditions, thereby reproducing the dominance of a certain subset of the social establishment. Schiffer (1981:903) does a service by calling attention to the weaknesses of such theories, no matter how they are conceptually packaged. However, it is arguable as to whether these theories can be “improved” by establishing a special set of methodological conventions to guide theory formation. As long as the thinking process is presumed to be ethically neutral, and concepts perceived as value-free, the empiricist project will likely continue to deliver nonrevolutionary theory and status quo archaeology.

## CONCLUSION

This chapter has offered an alternative set of philosophical issues to contrast with that recently suggested by Schiffer, one deriving from a particular understanding of dialectical materialism. Though an archaeological research agenda to accompany this epistemological position has not been specified here, the purpose has been more to outline the kinds of philosophical issues that emerge given a view of theory that differs radically from that held by Schiffer. Rather than espousing a concern for those factors that allow the successful use of extradisciplinary concepts or the generation of truly lawlike knowledge-claims, the concern raised here is for (1) why certain concepts are more popularly consumed by archaeologists than others; (2) what political, economic, and cultural conditions support the continued consumption of such concepts; and (3) what the consequences of this theory building are for other aspects of the scientific process and for the direction of society. In a time of widespread social and economic instability, such questions are at least as compelling as those raised by Schiffer. Such questions, however, lie outside the scope of empiricist epistemology. Alter-

natively, an appreciation of the epistemological position advocated here encourages archaeologists to consider how theory building—as a social process—influences those other social processes with which it necessarily articulates.

## ACKNOWLEDGMENTS

The following people offered helpful criticism on various drafts of this chapter: John Cross, Tom Leatherman, Bob Paynter, Martin Wobst, and Rick Wolff. The author accepts sole responsibility for final form and content.

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